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NOTE RELATING TO "WATER-HAMMER" IN STEAM PIPES.

By PROF. ROBERT H. THURSTON.

[A paper read before the American Society of Mechanical Engineers, June, 1883.]

The writer has been called upon during the past winter, in the course of his professional work, to examine into the systems of steam heating in use on so large a scale in New York city, and elsewhere, and to report upon the condition and capabilities of one of those "plants" which have been put in operation. In the course of this investigation he was compelled to examine into the peculiar methods of injury to which long lines of steam pipe thus used are exposed.

The action familiarly known as "water-hammer" had been long known to him, as to every engineer who has had much to do with steam power, and its singular effects had been often observed and commented upon by him, as by others; but, in this case, these effects were developed on so large a scale, and were so serious oftentimes in their consequences as to impress upon him the desirability of examining into the matter more carefully than had yet been done. He has not been able to make a systematic and thorough investigation, such as he would have liked to make; but he has been able to collect some facts, a few of which will be here presented, that may at least have the effect of calling the attention of engineers more generally to this matter, and may lead to further study of the subject.

When a pipe is filled with steam, and then has introduced into it a quantity of cold water, or when a pipe, itself cold, and containing cold water, even in very small quantity, and without pressure, has steam turned into it, the first contact of the two fluids is accompanied by a sudden condensation, which causes a sharp blow to be struck, usually at the point of entrance; and sometimes a succession of such blows occurs, which are the heavier as the pipe is larger, and which may be startling, and even very dangerous.

It is not known, so far as the writer is aware, precisely how this action takes place in all cases, or what conditions are most favorable to the development of the tremendous pressures which are often produced. Perhaps the action is as follows:

The steam, at entrance, passes over, or comes in contact with the surface of the cold water standing in the pipe. Condensation occurs, at first very slowly, but presently more quickly, and then so rapidly that the surface of contact between the two fluids is broken, and condensation is completed with a suddenness that produces a vacuum. The water surrounding this vacuum is next projected violently from all sides into this vacuous space, and crossing it, strikes upon the surface surrounding it. As water is nearly incompressible, the blow thus struck is like that of a solid body, and the intensity of the resulting pressure is the greater as the distance through which the portion of surface attacked can yield is less. In this manner enormous pressures are sometimes produced.

In some cases it seems certain that such pressures may be caused at points in the pipe far from either end, and from the point of entrance of the steam. For example, a pipe may lie in a nearly or quite horizontal line, and, if not fully drained, may contain a considerable quantity of water lying in the lower portion, while the steam may flow in above it. The passage of this steam along the surface of the water may cause some disturbance of the surface of the liquid, and this disturbance being gradually increased as the flow of steam becomes more rapid, may finally cause a break in the surface of the water, which disturbance may produce more rapid condensation and still further agitate the mass, until condensation occurs with such rapidity that a vacuum is formed at the point of greatest action. The next result is the rushing of steam from both directions towards this point, carrying with it, as it goes, masses of water which, coming from opposite directions with enormous velocity, meet at the intermediate spot at

which the condensation has been most rapid, and being stopped by instant collision, produce a pressure which may only have as its limit the strength of the pipe.

Where pipes are not burst by this action, it is common to see them sprung and twisted out of line, torn from their connections, and, when a succession of shocks occur, as is often the case, the whole line writhes and jumps lengthwise to an extent that is sufficiently serious to cause well-grounded alarm.

The writer had an opportunity, in the course of his work on the case above alluded to, to obtain some probably approximate measures of the intensity of this action in long and heavy lines of pipe. Four lengths of 8-inch pipe had been split by this action, and the writer desired to ascertain whether they remained, in their injured condition, strong enough to bear the ordinary steam pressure of the line from which they were taken. This never rose above fifty or sixty pounds per square inch. They were therefore subjected, in a proving-machine, to gradually increased pressures until the already fractured parts were still farther injured, the repairs, or rather the patching having been carefully done in such a way that they were not strengthened by it. This was done by putting on soft patches along the line of the split and securing them by bolts which were set in the line of the split. The patches thus served as simple stop-valves, preventing the exit of the water through the break.

The following are the results of the tests:

Pipe No. 1.—This pipe was split, near one end, for a distance of 15 inches, along the line of the weld. When placed in the proving-machine, it bore the applied pressure until it attained an intensity of 400 pounds per square inch, when the split suddenly extended about ten inches; the pressure could no longer be kept up, and the test terminated.

The pipe was then taken to a pipe-cutting machine, and the injured part cut off. It was then again subjected to pressure. It bore a pressure of 1,100 pounds per square inch,—the highest that it was convenient or customary to apply to that size,—and was taken out sound.

Pipe No. 2.—This length was cracked for a distance of 15 inches along the line of weld, not far from the middle of its length. The crack had opened a little and the pipe was slightly bulged. This piece bore 300 pounds and then gave way, the fracture extending just enough to let off the pressure.

At the opposite end of the pipe was another split, 8 inches in length. The part just fractured was cut off, and the remaining portion was again subjected to the water pressure. This time it bore 1,050 pounds per square inch, when the crack was started and ran about 15 inches. It began leaking, and showed plainly the effect of the pressure at about 800 pounds.

This was an unusually interesting specimen, as the pipe had been bulged considerably by the water-hammer along the line of the 8-inch crack. The pressure afterward borne, therefore, seemed to the writer to be likely to be a fair measure of that produced originally by the water-hammer. Such bulging as was here seen never occurs at usual pressures. The new break did not follow the weld, but ran irregularly, and apparently indifferently, through weld or solid iron.

Pipe No. 3.—This length was split for a distance of 22 inches, the end of the break being about 3 feet from the end of the pipe. It sustained a pressure of 250 pounds. The sound part of the pipe was then tested up to 1,050 pounds without injury.

Pipe No. 4.—This piece was split, like the last, and to just about the same extent, was tested similarly, and gave way at 300 pounds per square inch.

All of this pipe was 8-inch pipe, $\frac{3}{8}$ inch thick, and made with the usual form of lap-welded joint. The welds were not always perfect, as is probably the fact with all such pipe; but this pipe, butt-welded, would have borne very much higher pressures than those to which it was subjected in ordinary work by the steam carried on the line. It cannot be asserted that these lengths of pipe did not split under pressures less than those to which they were afterward subjected by the writer, as it is very possible that the first blow may have found a weak part of the pipe, and the split may, in some cases, have extended to stronger portions. Nevertheless, the writer is inclined to believe that this was not the case in all instances, and is convinced that, in one case at least—that of the 8-inch crack, which was accompanied by a decided bulge in the pipe—the water pressures, at the test, were, at least approximately, equal to, and are very likely to have exceeded, those obtained at the later test. It seems to him very certain that we may consider it as proven that the pressures produced by "water-hammer" are often enormously in excess of those familiar to us in the use of steam, and that they have in many cases exceeded 1,000 pounds per square inch. It is, then, evident that it is not often safe to calcu-

late upon meeting these tremendous stresses by weight and thickness of metal, but that the engineer must rely principally, if not solely, upon complete and certain drainage of the pipe at all times as the only means of safely handling steam in long pipes, such, especially, as are now coming into use in the heating of cities by steam led through the streets in underground mains.

The facts here presented have been, to the writer, something of a revelation, and have seemed to him to possess unusual interest and importance to the engineer using steam under such conditions as are here referred to. It is a fact which has long been well known, that these suddenly produced pressures are often very great. The writer has occasionally, for many years, known of serious and sometimes fatal accidents due to this cause; but that these stresses are often as great as is here indicated has probably been as little realized by engineers generally as by him.

Locusts in the Island of Cyprus.—The English government gives a penny and a half per pound for locust eggs. From July, 1881, to Feb. 1, 1882, 1329 tons of the eggs were brought to the English authorities and destroyed, each egg being much smaller than a pin's head.—*Rev. Scientif.*, Jan. 6, 1883. C.

Exploration of Greenland.—Nordenskiöld wishes to penetrate into the interior of Greenland, which, contrary to the general opinion, he thinks to be free from ice. This theory is based upon the fact that the *föhn* winds, or winds which are relatively dry and warm, must blow there. If such is the case, they cannot take into the interior of the country enough snow to feed a permanent glacier. He believes that vast regions covered with perpetual ice are a physical impossibility south of the 80th degree of North latitude. The illustrious voyager, with the energy and intrepidity which characterize him, does not hesitate to penetrate into the unknown deserts of the interior of Greenland, in order to test his theory. Among other studies, he proposes to continue those which he undertook in November last, upon the cosmic materials which the shooting stars bring to the globe. Those researches, in which he melted the snow collected at the surface of the ground, in order to examine its contents chemically, were not decisive; the residue consisted principally of feldspar and other terrestrial substances, without traces of metallic iron or nickel.—*Comptes Rendus*, March 5, 1883. C.

TABLE OF PISTON SPEEDS OF MARINE ENGINES.

By J. M. WHITHAM, Cadet-Engineer, U. S. N.

Name of Vessel.	Diameter of cylinders in inches.	Stroke of piston in feet.	HP (Total.)	Strokes of piston per minute.	Piston speed in feet per minute.	Remarks.
H. B. M. S. Inflexible..	70, 90, 90	4	8407	146	584	See King's "War Ships of the World."
" Neptune.....	118, 118	4½	8500	142	639	"
" Conqueror..	54, 70	3	4500	160	480	"
" Sultan.....	117·9, 117·9	4½	7736	139	625·5	"
" Alexandra.	70, 90, 90	4	8600	134	556	"
" Téméraire ..	70, 114	3⅞	7700	148	567·6	"
" Shannon....	44, 85	4	3540	140	560	"
" Nelson.....	60, 104	3½	6000	150	525	"
" Superb.....	116, 116	4	7430	134	536	"
" Dreadnought....	66, 90, 90	4½	8202	134·8	606·6	"
" Orion	65, 65	2½	4020	200	500	"
" Raleigh.....	100, 100	4½	6157	147·8	665·1	"
" Boadicea....	73, 93, 93	4	5523	150	600	"
" Iris and Mercury..	41, 75	3	7714	194⅜	583·1	"
" Conquest ...	36, 64	2½	2900	200	500	"
" Garnet.....	57, 90	2¾	2100	180	595	"
" Swift.....	38, 66	2	870	200	400	"
" Rover.....	72, 88, 88	4	4963·5	137	548	"
French, Admiral Dupené.....	61, 78⅜	39·4"	7397	154	505·6	"
Italian, Dandola.....	64, 120	4	.....	160	640	"
" Duilio.....	.....	3¼	7710	168	546	"
" Christoforo Colombo.	.....	37·5"	3782	172	537·5	"
German, Kaiser	114, 114	4	7890	154	616	"
I. R. S. Europe.....	42, 73	4¼	1800	130	552·5	"
" Asia.....	34, 56	3½	1050	132	462	"
" Africa.....	34, 60	4½	1350	136	612	"
" Zabiaca	34, 59	3	1470	184	554	"
" Novgorod.....	31, 45	2	499·6	200	400	See "Engineering," vol. xxi.
Austrian, Tagethoff.....	125, 125	4¼	8000	140	595	See King's "War Ships of the World."
Dutch, Atjeh.....	72½, 72½	2½	2670	154	336·6	"

Name of Vessel.	Diameter of cylinders in inches.	Stroke of piston in feet.	HP (Total.)	Strokes of piston per minute.	Piston speed in feet per minute.	Remarks.
Chilian, Almirante	46, 76	2½	2920	182	455	See King's "War Ships of the World."
" Huascar	54, 54	3	1650	156	468	"
U. S. S. Trenton	58½, 78, 78	4	3100	120	480	"
" Quinnebaug	42, 64	3½	1100	130	455	"
" Dispatch	33⅛, 34¼	2¾	403	129	356·6	See "Jour. of Franklin Institute," 1882.
" Miantonomah	32, 48 / 32, 48	3½	1600	130	455	See Edwards' "American Marine Engines."
" Essex and class	34, 54	3½	800	152	532	"
U. S. R. S. Rush	24, 38	2¼	266·5	141·68	318·78	See "Engineering," vol. xxi.
" Gallatin	34·1	2½	348	137·44	343·6	"
" Dexter	26	3	292	145·6	436·8	"
" Dallas	36	2½	234	126·8	317	"
U. S. F. C. S. Fish Hawk		2¼		206	463·5	See Qualtrought's "Pocket Manual."
S. S. Hudson	48, 48	6		138	828	See Edwards' "American Marine Engines."
" New Orleans	48, 48	5		140	700	"
" Knickerbocker	44, 44	6		140	840	"
" City of Pekin	51, 88	4½		106	477	"
" " San Francisco	51, 88	5		110	550	"
" Servia	72, 100, 100	6½	10350	106	689	See "Engineering," vol. xxxiii.
" Moor	51, 90	5	4250	139·5	697·5	"
" Aberdeen	30, 42, 70	4½	2086	131	589·5	"
" Sterling Castle	63, 90, 90	5½	8509	133	731·5	"
" Ohio	57¼, 90⅛	4	1977·5	120	480	See "Engineering," vol. xxii.
" Britannic	48, 83	5	5000	110	550	See Goodeve's "The Steam Engine."
Yacht Leila	9, 16	1½	149·9	443	664·5	See "Report to Bureau of Steam Engineer'g"
" Gitana	13½, 15, 15	1·3		500	666·6	See "Engineering," vol. xxii.
An English tug	14½, 25	1⅔		370	616·6	See "Engineering," vol. xxxiii.
Average piston speed for 50 vessels					544·006	
Locomotive "No. 5000"		2		700	1400	Built by the Baldwin Locomotive Co.
Italian torpedo boat		1⅓		960	1280	See "Engineering," vol. xxxiii.
H. M. S. Polyphemus		3¼	910	240	780	Designed speed. See same.
Small launch, named Bell Bird	8, 16	1		400	400	See "Engineering," vol. xxi.

U. S. Steamer Galena, Colonia, Uruguay, March 14, 1883.

LIQUEFACTION, VAPORIZATION, AND THE KINETIC THEORY OF SOLIDS AND LIQUIDS.*

By H. T. Eddy, Ph.D., University of Cincinnati.

In a solid, in which the molecules are evidently held at nearly fixed mean distances by cohesive and elastic forces, there are two kinds of partially constrained freedom of motion possible for each molecule as a whole; first a motion of its centre in a small orbit of more or less irregular shape about a mean position; and second, a more or less irregular pendular motion of oscillation about a mean directional position. Both of these motions can be properly treated as vibratory motions, and the laws of force under which the motions occur, though somewhat unlike, as will appear during the following discussion, have a general resemblance.

When a solid is liquefied, it is evident that the molecules slide on each other with facility, which is equivalent to supposing that the molecules of the body have become perfectly free as to directional position; and this may be explained by supposing that the pendular oscillation has been changed into rotation.

In order to gain a clearer conception of how the mean potential and kinetic energies have been effected by such a change, let us consider the case of a simple pendulum, which may be taken to be a heavy particle suspended by a thin rigid rod. It is well known that for any small oscillation of this pendulum the mean potential and kinetic energies are equal, but for oscillations of more considerable amplitude this is far from being the case. In order to compare the mean potential and kinetic energies for larger amplitudes, it will be useful first to make comparison in case of a body of unit mass falling freely through a height s under the action of gravity, in which case we must, in order to obtain the mean kinetic energy compute the mean value of $\frac{1}{2} v^2 = g s$ during the fall. Let the mean value be denoted by a horizontal stroke

$$\therefore \tfrac{1}{2}\overline{v^2} = \overline{gs} = \frac{g\int s\,dt}{\int dt} \qquad (1)$$

Since by the laws of falling bodies

$$s = \tfrac{1}{2} g t^2 \qquad (2)$$

$$\therefore \tfrac{1}{2}\overline{v^2} = \tfrac{1}{8} g^2 t^2. \qquad (3)$$

* Presented to the Section of Chemistry and Physics, O. Mech. Inst., April 26, 1883.

But the total energy potential and kinetic, which is evidently constant during the fall, is by (2),

$$gs = \tfrac{1}{2} g^2 t^2, \tag{4}$$

and this is, therefore, also the mean value of the total energy. We obtain the mean value of the potential energy by taking the difference of the means.

$$\therefore g\,(s - \bar{s}) = \tfrac{1}{2} g^2 t^2. \tag{5}$$

Hence by (3) the mean potential energy in a free fall is twice the mean kinetic energy. The same result holds in case of a body sliding down a smooth inclined plane. This result may then be applied to oscillations such as occur in the ideal case of a perfectly elastic particle which rebounds from a horizontal plane, whether vertically or obliquely, as is the case when it slides down one incline of a V and then ascends the other without friction. We are now in a position to see what is the relation between the potential and kinetic energies in case a smooth particle oscillates by sliding down one branch of a U and ascending the other. In the lower part of the curve the velocity is near its maximum value, and as this part of its path is more extended than before, the mean kinetic energy is greater in the U than in the V. Were the parts of the U which are nearly vertical put at a more considerable distance asunder, the mean kinetic energy could be increased at will by so doing. Now, it is known, in case the curve on which the particle oscillates is a cycloid, that the mean potential and kinetic energies are equal. Hence, in case it oscillates by sliding on any smooth curve, such as a semi-circle, whose vertical branches are not so far asunder as those of the cycloid which has a height equal to the radius of the circle, the mean kinetic is less than the mean potential energy. And it further appears that while for small oscillations the mean kinetic and potential energies are approximately equal, that as the amplitude of the oscillation increases, the mean kinetic energy is a smaller and smaller fraction of the total energy and of the mean potential energy. The same holds true in case the amplitude of the oscillation of the circular pendulum is pushed beyond 90° on each side of the mean. In fact, in case the kinetic energy at its lowest point is just sufficient to carry the particle to the highest point of the circle, the particle will rest there in unstable equilibrium, in which case the mean kinetic energy will vanish, since the time of oscillation is then infinite and the total energy will become potential and remain so. Thus we see that the

mean kinetic energy of the pendulum may be any fraction of the total energy less than one-half, according to the amplitude of the oscillation.

But there is one other circumstance of importance in this kind of oscillation, which is, that although its total energy is constantly increased as the amplitude increases, it is far otherwise with the mean kinetic energy. This at first increases with the amplitude, though not so rapidly as the mean potential energy, but as the amplitude approaches 180° the time of oscillation is so greatly increased that the mean kinetic is actually decreased by an increase of amplitude. There is, therefore, a certain amplitude at which the mean kinetic energy is a maximum. What that amplitude may be it does not now concern us to determine; the only point necessary to consider is the one stated, that an increase of total energy and a consequent increase of amplitude will in no case increase the mean kinetic energy unless the amplitude of the oscillation be carried to more than 180°. As has been seen, a considerable increase of total energy will serve only to diminish the mean kinetic energy, but if sufficient total energy be supplied to the pendulum, the amplitude will exceed 180° and its motion will be changed from that of oscillation to revolution, and on sufficiently increasing the total energy the mean kinetic energy of revolution will be equal to the mean maximum kinetic energy of oscillation.

The application of the foregoing discussion to the oscillatory motion of the molecules of solid bodies is obvious. In case a molecule has one or more positions of directional equilibrium about which it oscillates, a sufficient increase of total energy (*i. e.*, heat) will bring its mean kinetic energy of oscillation to a maximum, and in order further to increase its mean rotary kinetic energy a large amount of potential energy (*i. e.*, latent heat) must be supplied to change the oscillations into rotations of the same mean kinetic energy (*i. e.*, to melt the body).

There is one other point to be noticed in this connection, which is, that according to this theory the specific heat of a solid will in general increase as its temperature rises towards the melting-point, which is in general accordance with experiment. It would be necessary, however, in order that this should universally be the case, to show that the mean energy of the particular molecular motion which is the physical cause of radiant heat, is proportional to the mean energy of the motion which we are now discussing. It is generally thought that such a relation exists, and it is certainly highly probable that the ratio between them does not change rapidly with the temperature.

The results which we have here obtained are not due to the fact that the force causing the oscillation considered was a special function of the amplitude, or of the distance of the particle from its mean position. The only vital point is that the oscillation shall be such that there shall be a maximum value of the mean kinetic energy. This remark has special application to the phenomenon of vaporization also, which we shall now consider. In vaporization the molecules are evidently removed to such considerable mean distances from each other that their paths are mostly free from mutual interference, while, before vaporization, the molecules are in no part of their paths free from mutual action and reaction.

In considering the forces which act along a line joining two molecules between which cohesive forces act, the phenomena of cohesion and of resistance to compression make it evident that the force with which a molecule is drawn towards its mean position, when their mutual distance is slightly greater than the mean, is one which at first increases with the displacement of the molecule from its mean position, but the displacement cannot be large without carrying the molecules beyond the range of their mutual cohesion. If, however, their mutual distance be decreased, the force tending to restore a molecule to its mean position increases with the displacement, and in such a manner that no force can bring them into actual contact.

The cohesive force actually existing between the molecules of a liquid is so masked by the phenomena necessarily accompanying liquidity that its true character has been largely overlooked. There is, however, no reason, as we think, to suppose that the forces acting to prevent decrease of volume in a fluid are different initially from those resisting dilatation, or at least that they differ more than the same forces do in a solid body.

In order that we may make clear the relations between potential, kinetic, and total energy in case of vibration under the action of molecular forces of this character, we shall in the first place discuss the subject of the relation between the law of force and the distribution of potential and kinetic energy somewhat in detail. The law of force in case of the pendulum already discussed is

$$y = \sin x \qquad (6)$$

in which y is proportional to the force acting to restore the particle to its mean position, and x is proportional to the displacement of the particle from its mean position at the origin, measured along its path.

In case of a force varying as the direct first power of the displacement, the corresponding equation is

$$y = x, \tag{7}$$

which is the equation of a line tangent to (6) at the origin, and hence, as before seen, for vibrations of small amplitude the two laws are approximately identical.

Now let a third law be assumed to be represented by an equation of a form such as

$$y = \tan x. \tag{8}$$

Since (7) is also tangent to (8) at the origin, for vibrations of small amplitude, the laws of force expressed by (7) and (8) will cause vibrations in which the distribution of kinetic and potential energy is approximately the same. If, however, sufficient energy is imparted to the vibrating particle so that the amplitude is not small, it is not difficult to see that the mean kinetic energy no longer remains equal to the mean potential energy, as it is when the law of force is expressed by (7), nor is the mean kinetic energy the less of the two, as when the law is expressed by (6), but instead it is now the greater of the two. This appears perhaps most clearly from the consideration that this case more nearly resembles, than do the preceding, that of an elastic particle rebounding between two fixed walls, in which case the mean potential energy vanishes, and the mean kinetic energy is the total energy. Hence, it appears that when (8) is the law of force the ratio of the mean kinetic energy to the total energy is greater than one-half, and increases with the amplitude of the vibration. This is true not only for the law of force expressed by (8), but for any law which is represented by a curve passing through the origin tangent to (7), and falling on the same side of (7) as (8) falls.

From similar considerations it appears that whenever the curve expressing the law of force passes through the origin and is tangent at that point to (7), and falls on the same side of (7) as does (6), the mean kinetic energy of vibration will be less than half the total energy. Since in (8) the force of restitution becomes infinite at both $+\frac{1}{2}\pi$ and $-\frac{1}{2}\pi$, the oscillation is confined within those limits.

Let us now further suppose a law of force which will give a curve not having a point of inflection at the origin as does (6) or (8), but nevertheless tangent to (7) at that point.

Such a law is given by the equation

$$y = e^{-\tan x} \sin x, \tag{9}$$

if we take only that part of it lying between $x = +\frac{1}{2}\pi$ and $x = -\frac{1}{2}\pi$. On the negative side of the origin the curve somewhat resembles (8), while on the positive side, after reaching a maximum value of y at a point a little beyond $x = \frac{1}{6}\pi$, the curve rapidly approaches the axis of x, and the force vanishes at $x = \frac{1}{2}\pi$. This is in effect a combination of the two previous cases, and corresponds evidently to the case of the molecules of a solid or fluid held at mean distances from each other which are fixed at a given temperature.

During one part of the vibration the mean kinetic energy is more than half the total energy, as has been just now shown, while during the other half the mean kinetic energy is less than half that amount. It will depend upon the precise relation between the two parts of the curve what the effect is on the whole. But it is perfectly evident that for large amplitudes the whole effect will be of the same character as if both parts were of the kind expressed by (6), and the mean kinetic energy is then less than half the total. But it is to be noticed that for certain amplitudes which are not too large the contrary effect is quite possible, and the mean kinetic energy will be less than half the total energy. This is the kinetic explanation of the fact first established by Rowland,* that the specific heat of water slightly decreases as the temperature rises, from 0° C to about 30° C. A similar phenomenon has been observed in case of mercury.

But the most important conclusion which flows from the fact that the mean kinetic energy decreases with the amplitude is that in this kind of vibration also, there is a maximum value of the mean kinetic energy which cannot be increased by increasing the displacement unless the molecules be carried beyond the range of their mutual attractions. This is what occurs in vaporization with the resulting change of volume, and a large increase of the potential energy without increase of the mean kinetic energy.

As was shown in connection with the discussion of the oscillation of the pendulum, the character of the oscillation is unchanged when the value of x in (6) lies between $+\pi$ and $-\pi$, but entirely changes its character and becomes revolution if those limits are exceeded.

When the law of force is expressed by (7), the character of the

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*On the Mech. Equivalent of Heat, with subsidiary Researches on the Variation of the Mercurial from the Air Thermometer, and on the Variation of the Sp. Heat of Water. Reprinted from the *Proc. Am. Acad. Arts and Sciences*, Cambridge, 1880.

vibration is unchanged, however great the amplitude of the displacement; and similarly in case the law of force is that expressed by (8), the general character of the vibration cannot be changed, however large the total energy imparted to the vibrating particle, and in this case the amplitude of the vibration evidently cannot under any circumstances be increased beyond $\frac{1}{2}\pi$, but x must always lie between $+\frac{1}{2}\pi$ and $-\frac{1}{2}\pi$, so that as the amplitude approaches these limits, the motion resembles more and more that of an elastic particle rebounding between a pair of immovable parallel walls at a distance π from each other.

When, however, the law of force is that expressed by (9), the amplitude of the vibration cannot exceed $-\frac{1}{2}\pi$ in one direction under any circumstances, but in the other direction the amplitude cannot exceed $+\frac{1}{2}\pi$ without a change from vibration to free motion.

The phenomena to be represented show that the same general relation holds for any law of force of which the curve has the general characteristics of (9), such as, for example, that part at the right of the origin of the curve

$$y = ax^{-2} - bx^{-5} \tag{10}$$

in which y is the force and x the distance between the molecules. Such a curve as (10) must represent the facts of nature more accurately than the relation between them proposed by Maxwell, and expressed by the equation

$$y = -bx^{-5} \tag{11}$$

which was taken by him to express the force acting during an encounter. But the law of force during an encounter cannot be supposed to differ from that existing during any state of nearness of the molecules, so that (11) is to be regarded merely as an approximate expression for a law like that of (9) or (10), in which the part of the ordinate of the curve expressing the attraction of the molecules is neglected. It must be noticed that the origin in (10) and (11) is differently situated from that in (9), as will be seen upon sketching the curve. The line $x = -\frac{1}{2}\pi$ in (9) is that taken as the axis of y in (10) and (11).

Having now considered in general the nature of the change which occurs in the molecular motion during vaporization, a somewhat closer consideration will enable us to see the remarkable mechanical significance of the so-called critical temperature, as well as that of ebullition. In ordinary evaporation into the atmosphere, without ebullition only those molecules escape from the surface, whose kinetic energy is such as

to enable them to overcome cohesion, and only so many can escape as will enable the vapor to fulfill approximately the law of Gay Lussac. The process by which the vapor tension in this case acquires its maximum value, is one whose velocity is regulated by the laws of gaseous diffusion, when the vapor tension is less than the atmospheric pressure. But when the temperature rises to such a degree that the mean kinetic energy of the molecules (which according to our theory may be the same in both liquid and vapor) is greater than that of the superincumbent atmosphere, the vapor no longer merely diffuses from the surface, but is able mechanically to push back the atmosphere, or other pressure, to which it is subjected, and ebullition takes place. Even in the case of ebullition, however, only part of the molecules are able to free themselves from the force of cohesion, namely, those molecules which happen to have a kinetic energy so far above the mean as to carry them out of the range of the force of cohesion, and enable them to push back the superincumbent pressure. If, however, the temperature be still further increased, a point must at last be reached, whatever be the volume and consequent pressure, such that the mean kinetic energy is greater than the maximum possible in the liquid state. The liquid must then become a gas without regard to volume or pressure. The temperature at which this occurs is the critical temperature for the liquid considered.

There are then three kinds of vaporization possible, evaporation, ebullition, and gasification.

There appears, however, to be only one kind of liquefaction, (melting) which is most nearly analogous to gasification, and without further reasoning it will appear that for those substances whose melting point is lowered by pressure there must be a critical temperature of melting above which the body will be liquid, whatever be the pressure, *i. e.* no pressure can lower the melting point beyond a certain amount dependent upon the constitution of the body.

The results of these views as to the mechanics of liquefaction and vaporization are in general accordance with the hypothesis of Clausius,* who supposes that the mean kinetic energy or "real specific heat" does not depend upon the state of aggregation of the body, and they are in exact accordance in this particular that the mean kinetic energy need not be changed by change of state from solid to liquid or from liquid

* *Philos. Mag.* Series [4], 24, 1863.

to gas. But whether the translatory motion of the molecules is the direct physical cause of radiant heat is still an open question which we shall discuss more at length in another place, where we shall also take occasion to discuss the relation between the specific heats of a body in its different states of aggregation, solid, liquid and gaseous.

There is, however, one point which may perhaps be well considered in this connection, and that is the explanation of the very large specific heat of liquids, compared with the same substances in the solid or gaseous state. This appears to be very probably due to the effects of the centrifugal force developed in the freely rotating molecules. The law of most probable distribution of rotary velocities shows that although the larger number of molecules are rotating with a velocity whose square is near the mean, yet all other velocities also exist, and among them very great ones.

Those rotary velocities which are sufficiently great will dissociate the atoms of the rotating molecules from each other, and the energy of rotation being expended against atomic forces will become potential and will not again become an available part of the kinetic energy until the atoms so separated meet others with which they can reassociate into molecules. A certain small per cent. of the total mass of the liquid must continually be thus dissociated in order that atoms may meet sufficiently frequently so that reassociation may be possible, and a uniform condition exist, in which as much reassociation happens as dissociation. This quantity must increase with the temperature, and the energy thus employed is apparently the reason of the high specific heat of liquids. In electrolysis these dissociated molecules are mechanically separated from the others, and to keep up the per cent. of dissociation a new supply is required for which energy must be supplied, just as it must be to preserve the temperature of a fluid from whose surface vapor is constantly being removed. A somewhat analogous phenomenon occurs in vaporization, but as it only can occur ordinarily at the surface, the analogy has been overlooked. A liquid gives off vapor at temperatures below boiling. According to the theory broached in this paper, only those molecules pass beyond the range of the cohesive forces whose kinetic energy is sufficiently above the mean to enable them overcome the cohesive force and have still remaining on the average an amount of kinetic energy equal to the mean.

But this can only occur at the surface, for inside the mass of the fluid such molecules are entangled and soon lose this superfluous energy

in encounters with other molecules. We may perhaps put the comparison in a clearer light by supposing a liquid, in which no such dissociation occurs, such as mercury probably is, and let it be in connection with a large closed space which is filled with the vapor of the liquid at its maximum tension. Now raise the temperature of the fluid and wait until the vapor in the closed space has reached the tension corresponding to this temperature. The heat supplied is partly employed in increasing the kinetic energy, and partly in vaporizing a portion of the liquid, and is much larger in amount than would be required if there was no vaporization. In a quite analogous manner, the heat energy necessary to raise the temperature of water would probably be much less were there no dissociation accompanying the increase of temperature.

It appears from experiment that the specific heat of mercury is nearly the same in its solid and liquid states as it should be in case its molecules are monatomic in both states. It would be of great interest to know whether the specific heat of mercury gas has also the same value. It is noticeable that the latent heat of liquifaction of mercury is extremely small, which would lead to the hypothesis that the atoms of mercury are very nearly round and smooth, and that the rotary energy of a molecule of mercury but a very small fraction of its total kinetic energy.

Cadmium, which is monatomic as a gas, would also furnish an interesting test of the theory, for it is not likely that the molecule of a substance greatly changes its character by change of state. Dissociation of the atoms of a molecule is evidently due to the motion of atoms away from each other. It is yet a question as to whether such oscillations can be caused in the atoms of a molecule as to absorb an appreciable amount of energy, or of such intensity that they can be caused to rotate. If such rotations are ever caused within the molecule itself, they probably occur at temperatures far above vaporization.

The conclusions to be drawn from the specific heats of elementary substances whose molecules consist of but two like atoms are in general confirmatory of the theory proposed in this paper, for the amount of rotary energy for such substances should be a less fraction of the total energy than in bodies consisting of a larger number of atoms, and the per cent. of dissociation should also be less. Now, in fact, for such bodies the specific heat in the liquid state does not, in general, greatly exceed that in the solid.

ABSTRACT OF PAPER ON THE COMPARISON OF INDICATOR RIGS.

By Robert Grimshaw.

[Read before the Franklin Institute, May 18, 1883.]

Correct instruments may be applied and used in wrong ways, and give incorrect results.

The steam engine indicator is frequently so applied and used; the devices employed to reduce the motion of the crosshead often causing great errors in the diagram.

Among the methods we may consider

(1) Cord attachment from a pin in the end of the shaft or side of crank, direct to the paper barrel.

(2) Cord from a swinging lever slotted to receive a pin in the crosshead.

(3) Cord from a swinging lever connected with the crosshead by a vibrating link.

(4) Cord from a swinging lever attached to the crosshead; the pivot of this lever vibrating at the end of a swinging link.

(5) The reducing wheel.

(6) Pantograph.

(7) "Lazy Tongs."

In devices 2 and 3, which are the most commonly used, the cord may be either

(*a*) Attached directly to the lever.

(*b*) Wrapped around a circular arc or sector centered in the pivot of the pendulum.

No. 1 is irrevocably tainted with the angularity of the connecting-rod, compounded with that of the cord. It always gives less than $\frac{1}{4}$ the diagram-length, for the first quarter of the out-stroke of the piston; and gives longer cards on an indicator at the back end than at the crank end. The shorter the connecting-rod and cord, for a given stroke, the greater the errors.

(2) The slotted lever gives absolutely correct results when at mid-stroke, it is perpendicular to the crosshead path, and when,

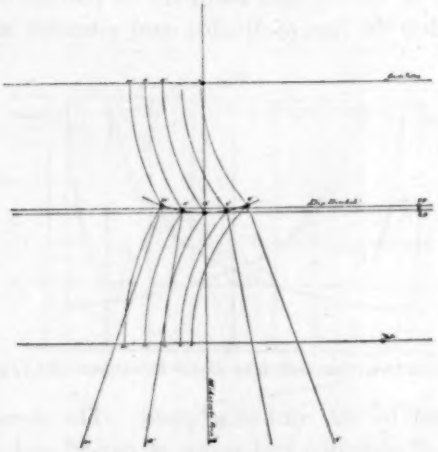
(*a*) The cord gets its motion from a pin which is driven by a radial slot centered in the pivot of the lever, and perpendicular to the cord at mid stroke.

(*b*) With long cord attached directly to the lever the results are practically correct, provided that the "dip" of the cord is divided, and the cord is led off so that at mid-stroke it makes a right angle with the radial line from its attachment to the pivot.

(*c*) With a long cord wrapped around a circular arc, centered in the pivot, the results are practically correct, no matter how the cord is led off.

This theoretical, but practically immeasurable error, gives equal cord lengths for both halves of the stroke, but the end quarters give somewhat longer card lengths than the central quarters. The error decreases with the total angles of lever and cord swing.

Fig. 1.



Motion of Usual Pendulum and Link.

(3) The lever with fixed pivot, and linked to the crosshead, gives quite good results when properly applied, although not so accurate as a double-slotted lever of the same length.

At mid-stroke it should be vertical, and the link sufficiently below the crosshead attachment to divide the dip.

Remarks *b* and *c* concerning the cord attachment to the slotted lever apply to this rig. As in the latter rig also, the longer the lever, and the cord between lever and guide pulley, the more accurate the results.

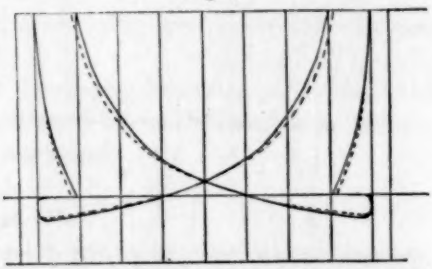
Fig. 1 is reduced from a carefully made plotting, full size, of the motion of a rig with levers 36 and 24″ long. The vertical line represents the position of the pendulum at mid-stroke. The dip of the

link was divided at this position. Four different cord angles gave as many varying results. When the angle of cord vibration was divided and the guide pulley placed at the point *GP* on the second horizontal line parallel to the crosshead path, the error in a 4″ card was hardly measurable. Circular arcs swept from the upper point *GP*, from the points indicating successive positions of the point of cord attachment, show that with this position of guide pulley the first quarter of the forward piston stroke gives more, and the last quarter less, than a quarter of the card length, this being reversed on the back stroke.

Carrying the guide pulley below the line of divided cord vibration, the errors of card length are exactly the opposite of those made with the guide pulley above the line of divided cord vibration.

Fig. 2 shows in dotted lines the result of putting the guide pulley even so little below the line of divided cord vibration as the bottom of

Fig. 2.



False Diagrams taken by Usual Pendulum and Link.

the arc described by the cord attachment. The correct cards for the plotted amount of clearance and points of cut-off and compression are in full lines.

(4) The pendulum with swinging centre is probably the most erroneous used or proposed. Fortunately it is not a usual rig. It does not permit of the use of a circular sector for cord attachment, nor can the cord be attached, as in where the pendulum pivot is stationary, anywhere but in the line of centers of the long lever; thus losing one of the main advantages of fixed centre levers, the dispensing with a guide pulley.

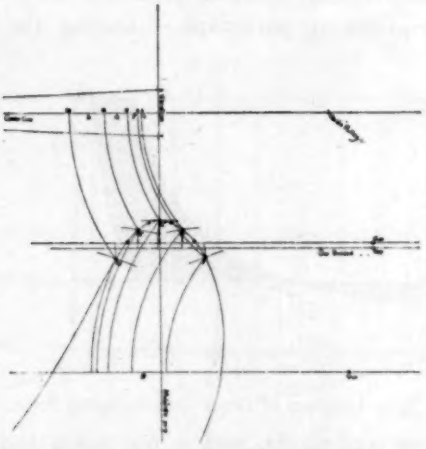
The point of cord attachment describes a mixed curve which is neither regular nor symmetrical, one limb being variably convex, and part of the other being decreasingly convex and then increasingly concave.

With the lever plumb at mid-stroke, the "dip," or versed sine of the swing of both levers divided at mid-stroke, and the cord running off parallel to the crosshead path, the results, while not so correct as those from the other lever rigs, and not accurate enough to set valves by, will do to give a general idea of engine performance and an approximation as to horse-power.

The greater the angular vibration of pendulum, link and cord, and the greater the angle made by the cord with the path of the crosshead, particularly where the cord makes an acute angle with the radius between cord attachment and swinging centre, the greater the error.

This tendency to error is so great, that some cord angles likely to

Fig. 3.



Motion of Lever with Vibrating Pivot.

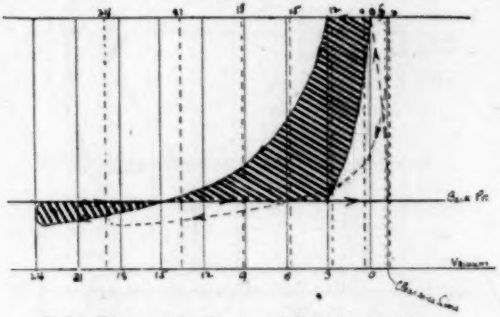
be chosen by any one not thoroughly conversant with the errors of this rig, actually give a negative movement to the paper drum, during the first portions of the outstroke; so that for an engine with a lazy steam valve the admission line would appear as compression to initial pressure, and with cut-off at $\frac{1}{16}$ stroke the diagram would commence with the expansion curve and have two vacuum loops so large as to prove the engine to be driven by the main belt and pumping into the boiler.

Fig. 3 is a photo reduction from a plotting made with scrupulous care, with a 36″ pendulum swinging on a 24″ link, the "dip" or versine of the latter being divided by the vertical line, which represents

position at mid-stroke. The curved trace was made by inserting a pencil point in the hole for cord attachment; the line being the "raised curve" referred to in a previous paragraph. The nine small circles on this curve show the successive positions of the cord attachment at equidistant portions of the piston stroke. With the angle of cord vibration divided, the error on a 4″ card is measurable, but not serious for rough calculations of horse-power, though sufficient to throw the cards out for plotting true theoretical expansion curves. The slightest variation from this height of guide pulley produces an error so great as to vitiate any cards with early cut off, even for horse-power calculation.

With the guide pulley much above the line of divided card vibration, as shown in the diagram, there is produced the astounding result referred to in a preceding paragraph :—During the first eighth of a

Fig. 4.



False Diagram by Lever with Swinging Pivot.

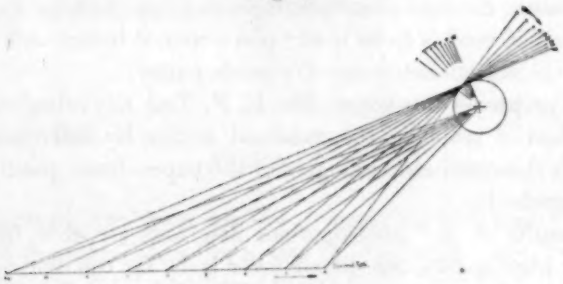
forward stroke the cord slacks, and is not again tightened until the end of the third eighth of the stroke. The positions of the point of cord attachment at successive eighths of the forward stroke are shown in Fig. 3; while in Fig. 4, the dotted lines show the false cord which would be produced with 60 lbs. absolute initial pressure, and cut off at $\frac{1}{8}$ stroke (clearance and compression being as shown in the correct theoretical diagram hatched in full lines). The numbers 0, 3, 6, etc., on the lower line indicate correctly successive piston positions; the same numbers on the upper line showing where the piston would be represented to be, by the rig shown.

(5) The reducing wheel gives absolutely correct results when properly applied, which is seldom the case. The cord should be led from an offset from the crosshead, so that it will, at all piston positions, be

absolutely parallel to the piston rod. When it makes an angle with the rod, it gives a shorter card than due to the stroke, but not only that, the card for any given fraction of the out-stroke is shorter than that for the succeeding one.

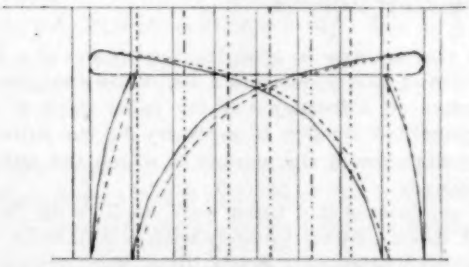
These errors being due to the decreasing angle of the cord as the piston advances, it follows that if the indicator is screwed into first one end of the cylinder and then the other, the diagram from the crank end will be not only shorter than that from the back, but more distorted; and that the further the wheel is from the central line of the

Fig. 5.



Motion with Wrongly Applied Reducing Wheel.

Fig. 6.



False Diagrams taken by Reducing Wheel.

engine, and the nearer the crosshead attachment to that same axis, the greater the error.

False cards by this rig always show the cut-off too early and horse-power too low on the out-stroke, and the reverse on the return.

In Fig. 5 there are plotted eight equal successive portions of a 24″ forward piston stroke, and a 4″ reducing wheel wrongly applied. The angles made by the tangents representing the successive positions of cord, are greatest for the earlier equal portions of the outward stroke;

so that instead of correct diagrams like the full lines in Fig. 6, three are taken such as are shown by the dotted lines; the right hand card being from the back end.

(6) The pantograph can be made to give perfect cards, the only danger being in oblique cord connections.

(7) The same remarks apply to the lazy tongs; but in this there appear two special sources of error: lost motion in a multiplicity of joints, and springing of the frame at high speeds, prolonging the card at the two ends, hence making cut-off appear too early and horse-power too low.

In all cases, the cord should be light and non-stretchable. Braided cord is best, if cord is to be used; piano wire is better, and absolutely prevents the objectionable use of a guide pulley.

[Since preparing this paper, Mr. B. F. Teal has submitted a model of a method of reducing the crosshead motion by differential pulleys carried on the crosshead, and giving the paper-drum positive motion on each stroke.]

The results of my investigations and tests are, that for accurate results at high speeds, the slotted lever is by far the best rig, from its simplicity, cheapness, ease of attachment while engine is running, lack of spring, direct cord connection, and adaptability to various lengths of stroke and position of indicator.

[Before this abstract was in type, the appearance of a lengthy mathematical discussion of this question in a well-known magazine, by a gentleman who commenced a discussion of the paper itself at the meeting at which it was presented, renders it necessary for the writer to add to the abstract a memorandum of the manner in which the tests and measurements were made.

Hard wood pendulums and links were made, with the pivot centres accurately laid down, drilled squarely with a $\frac{1}{16}$th twist drill, and their distances carefully measured. Scribe lines were drawn and carefully drilled in the same manner to mark successive eighths of 24″ piston strokes. With a tested T square, the mid-stroke positions of the levers were laid down previous to drilling the centres for piston position holes. The levers were then accurately centred, and the point of cord attachment drilled for a metal-marking point which should not only make a trace of the entire motion-curve of the point of cord attachment, but would prick the positions of that point at each piston position. The distances from guide pulleys were transferred by stiff trammels, and the card lengths measured to hundredths of an inch as well as graphically recorded.

In addition to the graphical tests and proofs made by the levers themselves, full-sized diagrams were made with a hard pencil, and these were measured to check the graphical records.

In still further corroboration, an indicator was clamped to the drawing-board in various positions, and the successive eighths of piston stroke recorded on the usual paper card by direct cord attachment to the levers.

The levers, thus connected to the indicator, were shown at the Institute meeting, and cards from the "swinging-pivot" lever were taken by the author, and handed out at the meeting, corroborating those portions of the paper which referred to that rig. After the meeting, the writer offered the indicator to the member who upheld that rig, if he would either take a correct card with that rig, or disprove the methods and measurements there offered. The author still owns the indicator.

The writer's measurements, particularly those of the swinging-pivot lever rig, were checked by skilled engineers and draftsmen in various parts of the country, and the levers and board are on deposit at the Franklin Institute, for the convenience of those who may wish to test for them-

ERRATA.

Page 22, third line from top, for "raised curve" read "reversed curve;" on fourteenth line from top, for "a" read "the;" on tenth line from bottom, for "cord" read "card."

Page 24, first line, for "three" read "there."

Page 25, formula should read: "Given, $a = b$; then, $a^2 - b^2 = ab - b^2$; $(a+b)(a-b) = b(a-b)$; $a+b = b$; $2 = 1$."]

of zinc is less, and there is very little silicon. It differs [illegible] was used for four days without disturbing the mounting and was employed every evening to produce electric light.—*Comptes Rendus.* C.

Magnetism of Rotation.—Tresca has reported to the French Academy some experiments by Selim Lemström upon the causes of terrestrial magnetism. He used a paper tube with two concentric walls, rotating rapidly around a cylinder of soft iron, and his results confirmed the theories of Edlund and Chase, which explain magnetic effects by æthereal movements. The experiments were substantially the same as those which were performed by Chase in 1864 and 1865, and reported in the ninth and tenth volumes of the Proceedings of the American Philosophical Society.—*Comptes Rendus.* C.

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The writer's measurements, particularly those of the swinging-pivot lever rig, were checked by skilled engineers and draftsmen in various parts of the country, and the levers and board are on deposit at the Franklin Institute, for the convenience of those who may wish to test for themselves.

The inference is clear: Where carefully made measurements by distant and disinterested parties give different results from those obtained by calculation, there must be a leak in the mathematics.

In this connection, while not in the least wishing to disparage mathematical investigation and its results, the author begs to offer the following:

Given, $a=b$; then, $a^2b^2=ab-b^2$; $(a+b)\ (a-b)=b\ (a-b)$; $a+b=b$; $2=1$.]

Improvement in the Bunsen Battery.—M. Azapis substitutes for the acidulated water a solution of about 15 per cent. of cyanide of potassium, caustic potash, common salt, or sal ammoniac. The liquid in the porous vase which receives the carbon is common nitric acid, the same as in the Bunsen cell. The intensity is equal to Bunsen's; the zinc does not need amalgamating; the constancy is greater; the waste of zinc is less, and there is very little smell. A battery of 25 elements was used for four days without disturbing the mounting and was employed every evening to produce electric light.—*Comptes Rendus.* C.

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INVESTIGATION OF THE CIRCUMSTANCES AFFECTING THE POTABILITY OF THE SCHUYLKILL WATER SUPPLY IN THE MONTH OF JANUARY, 1883.

EXECUTED UNDER THE AUTHORITY OF THE BOARD OF EXPERTS AND THE CHIEF ENGINEER OF THE WATER DEPARTMENT OF PHILADELPHIA, BY PROF. ALBERT R. LEEDS, PH. D.

LABORATORY OF CHEMISTRY.

Stevens Institute of Technology,
Hoboken, N. J., February 20, 1883.

To MESSRS. FREDERICK GRAFF, J. VAUGHN MERRICK, Members of the Board of Experts, and COLONEL WILLIAM LUDLOW, Chief Engineer of the Philadelphia Water Department.

GENTLEMEN:—I transmit herewith my Report containing the results of an investigation into the causes affecting the potability of the Schuylkill River water, during the month of January of the present year. It should be premised that this investigation largely excluded those causes which could be determined by direct observation, such as impurities carried into the river by surface and subsoil drainage, by sewers, by the refuse of factories, from cesspools, from the spent liquors of gas works, from the irruption of animal matters derived from slaughter houses, etc., etc., and was principally confined to the study of those causes which might be discoverable by physical and chemical examination of the waters themselves.

Though this restriction, which was due to the limited scope of my official instructions is much to be regretted, yet it necessitated a more thorough search within the narrow field of laboratory analysis and experiment open to me. It is also less to be regretted for the following reasons:

The *fact* of the principal source of the Philadelphia water supply, the Schuylkill, being at the present time a sewer and factory-polluted stream, was conceded. It was also well established that the actual potability of the water at any particular season, is determined:

1st. By the ratio then existing between the volume of flow and the volume and character of polluting materials, a small amount of contamination when the volume of water is at a minimum, possibly dam-

aging the potability far more than the maximum amount of pollution existent along the banks of the Schuylkill, when its flow is also at a maximum.

2d. By the relative energy of the agencies (mostly artificial) at work to contaminate, and the agencies (almost, if not entirely natural) operating to purify the stream. If these natural agencies are in full sway, they may dispose of a large amount of impurities and bring the water back into such a condition as neither to affect the sense of smell or taste, or exert a detrimental effect upon health.

There are ample grounds for believing that, up to the present time at least, this balance of opposing forces has usually inclined to the favorable side in the case of the Schuylkill water, and that therefore it has been entitled to be ranked as a wholesome water supply. Similar conclusions were arrived at by Professors Booth and Garrett, and by Dr. Charles M. Cresson, in their elaborate reports appended to the "Report of the Commission of Engineers on the Water Supply of Philadelphia, 1875." On the other hand, if their operation is least active on account of an unpropitious season of the year, or stagnant conditions of the water, or possibly of a covering of ice, they may be unable to dispose of a small quantity of polluting material, and at such a time the water supply may become most offensive.

I shall not anticipate, however, results obtained and conclusions based thereon, but shall present in detail the work actually performed. This may be most concisely done under the five following heads:

1st. Collection of the samples.

2d. Methods of analysis.

3d. Interpretation of the data obtained by analysis and experiment.

4th. Light thrown by the results upon the origin of the January taste and smell.

5th. Questions relating to the deterioration of the water supply.

1. COLLECTION OF THE SAMPLES.

During the latter part of the period when the water was in its most nauseous conditions, by the authority of the Water Commission and Chief Engineer, Dr. Wm. H. McFadden, the following samples were collected and forwarded to me for analysis.

I. Schuylkill river at Pottstown, January 10, 1883. (Laboratory No. 226.)

II. Schuylkill river at Roxborough, January 10, 1883. (Broken in transportation.)

III. Schuylkill river at Schuylkill Works, January 9, 1883. (Broken in transportation.)

IV. Schuylkill river at Norristown Works, January 10, 1883. (Laboratory No. 227.)

V. Schuylkill river at Fairmount Works, January 9, 1883. (Laboratory No. 228.)

VI. Schuylkill river at Belmont Works, January 9, 1883. (Broken in transportation.)

VII. Schuylkill river at Fairmount Works, January 13, 1883. (Laboratory No. 229.)

Subsequently, being desirous that my investigation should extend beyond the determination of the mere fact of contamination, and should be directed to the discovery, if possible, of the origin of the bad taste and smell on this particular occasion, the Chief Engineer desired me to visit the Schuylkill river in person, and collect such additional samples as would be of value.

In accordance with these instructions, January 19 and 20 were spent upon the ice, then covering the river, in company with Assistant Engineer, C. G. Darrach, whose aid was of the greatest service in carrying out the proposed line of inquiry.

In the first place it was deemed important to ascertain whether there was a difference of temperature between the water at the bottom of the river and at its surface, or rather immediately under the ice. If such proved to be the case, there would be a corresponding difference of density, and possibly a difference in composition. To make these determinations, a bottle, containing a thermometer, was properly weighted so that it might sink in a vertical position until the bottom of the bottle came to within about three inches from the bottom of the stream. The stopper was then pulled out, and after filling, the bottle was quickly pulled to the surface and the temperature noted. The temperature of the surface samples was also observed whilst the thermometer was immersed in the bottle, this precaution being taken to exclude errors arising from atmospheric influence. In this manner the following six samples were obtained.

SAMPLES COLLECTED IN PERSON JANUARY 19 AND 20.

No. 230 Surface water, Fairmount Basin, January 19.

No. 237 Bottom water, Fairmount Basin, January 19.

No. 233 Surface water, Spring Garden Basin, January 19.

No. 232 Bottom water, Spring Garden Basin, January 19.
No. 235 Surface water, Roxborough Basin, January 20.
No. 234 Bottom water, Roxborough Basin, January 20.

The observed temperatures were as stated; the specific gravities at 15° were subsequently determined with great care in the laboratory, and are likewise given.

No.	Temperature Air.	Temperature Water.	Specific Gravity at 59° F.
230	34°	33°·2	1·001468
231	34°	33°·8	1·001477
233	34°	33°·4	1·001471
232	34°	33°·8	1·001471
235	36°	33°·	Undetermined.
234	36°	33°·4	"

Two points deserve comment with regard to these determinations:

1. The average density of the Schuylkill water at the time when the samples were taken, when reduced to the mean temperature of 59° F., was very approximately 1·00147, and inasmuch as the volume of pure water, according to Kopp, diminishes from 0·999966 to 0·999947, when its temperature is raised from 33·4° F., to 33°·8, the density of the bottom sample, No. 232, at the time it was collected, was 1·0022202, while that of the surface sample, No. 233, was 1·0022081. There is therefore in winter, when the river is covered with ice, a current of colder water flowing immediately beneath the ice, and another current, of warmer water, but of greater density and probably flowing much more slowly, at the bottom of the stream.

2. The temperature of the water in the Schuylkill in midwinter, and when the river had been for some time entirely covered with ice, was about one Fahrenheit degree above freezing point at the surface, and about 1·4 above freezing point at the bottom, or four-tenths of a degree warmer.

2. METHODS OF ANALYSIS.

Inasmuch as a knowledge of the methods employed is essential to the proper understanding and interpretation of the analytical results, it is necessary to state briefly what these methods were.

FREE AND ALBUMINOID AMMONIA.

The method of Wanklyn, Chapman, and Smith, performed in accordance with certain modifications described at length in the *Zeitschrift für Analytische Chemie*, xvii, p. 276, was made use of. These modifications are, in brief, the employment of a Color Comparator, which admits of the comparison of the test with the standard under conditions most favorable to accurate comparison. Secondly, the substitution for the various standard solutions, which have to be freshly prepared for each analysis, of a standard wedge graduated once for all with extreme care. This wedge can be used for months without its scale undergoing alteration, and its employment insures uniformity between the results of analyses performed at different times, and greatly shortens the length of time required for the performance of long series of analyses.

OXYGEN REQUIRED TO OXIDIZE THE ORGANIC MATTERS, OR "REQUIRED OXYGEN."

This was determined by potassium permanganate in acidified solution, the samples being plunged for exactly 10 minutes in water at 98°. All of the samples were treated in precisely the same manner, and a correction was applied to each result, amounting to the value of the alteration of the reagent, when tested upon distilled water under identical conditions. After many unsuccessful attempts to obtain concordant results by this method, I had recourse to a new one, devised during the course of this investigation, and which is founded upon the reduction of silver salts in solution by organic matter in sun-light. This is not the proper place to give the details of the new "Actinic Method," nor the experiments which led me to abandon the potassium permanganate test, the publication of these details being necessarily left to the chemical journals. I have given, in the accompanying table, the results obtained by the use of potassium permanganate for samples 230 to 235, and by the Actinic Method for same samples. It will be noted that the figures obtained by the Actinic are higher than those obtained by the Permanganate process. My own conclusions in the present investigation are based on the results obtained by the former method.

REQUIRED OXYGEN.

No.	PERMANGANATE PROCESS.	ACTINIC PROCESS.
230	0·18 parts per 100,000.	0·218 parts per 100,000.
231	0·19 " "	0·244 " "
233	0·18 " "	0·299 " "
232	0·18 " "	0·283 " "
235	0·16 " "	0·206 " "
234	0·16 " "	0·231 " "

Nitrites.—The nitrites were determined by means of hydrochlorate of naphthylamine and sulphanilic acid, according to the reaction discovered by Griess, and which is so sensitive that it permits of the easy recognition of one part of nitrous acid when diffused through a thousand million parts of water. They were calculated as nitrous acid (HNO^2).

Nitrates.—These were determined upon a deciliter of the water, after evaporation to dryness, subsequent heating in presence of lime, reduction in the cold by a copper zinc couple, and complete distillation of the formed ammonia. They were calculated as nitric acid (HNO^3).

Total Solids.—These results express the weights of the residues obtained by the evaporation of a deciliter and drying to constant weight at 100°C.

Dissolved Oxygen.—The number of cubic centimeters of oxygen dissolved in a liter of water was determined by Mohr's method, which is based upon the relative amounts of oxidation of protoxide of iron to the form of peroxide, effected by the various volumes of oxygen held in solution.

The results obtained by this process are closely concordant among themselves, and whilst they do not possess the same degree of absolute accuracy as those obtainable by the laborious eudiometric methods of Bunsen, yet, Mohr's method has the great advantage that it admits of the determinations being made upon a number of samples before such an interval has elapsed, that the constitution of the water and dissolved gases have possibly undergone considerable change.

Acidity and Alkalinity.—To determine whether the water possessed an acid or alkaline reaction, and the actual amount of the acidity or alkalinity as the case might be, centinormal standard solutions were employed with alizarine and phenolphthalein as indicators, according to the method given in the *Jour. Amer. Chem. Soc.*, ii, p. 71.

Sanitary Analysis of the Philadelphia (Schuylkill River) Water Supply, 1883, by Prof. Albert R. Leeds, Ph.D.

	No. 226—Pottstown, January 10.	No. 227—Norristown, January 10.	No. 228—Fairmount forebay, January 9.	No. 229—Fairmount forebay, January 13.	No. 230—Fairmount basin, at surface, January 19.	No. 231—Fairmount basin, at bottom, January 19.	No. 232—Spring Garden basin, at surface, January 19.	No. 233—Spring Garden basin, at bottom, January 19.	No. 235—Roxborough basin, at surface, January 20.	No. 234—Roxborough basin, at bottom, January 20.
Free ammonia.										
Parts per 100,000	0·009	1·0125	0·0195	0·02075	0·0165	0·01575	0·013	0·01775	0·017	0·01325
Grains per gallon	0·0052	0·0073	0·0113	0·0111	0·0096	0·0091	0·0078	0·0103	0·0099	0·0077
Albuminoid ammonia.										
Parts per 100,000	0·0145	0·0125	0·02375	0·01425	0·01275	0·01425	0·01175	0·01375	0·015	0·01425
Grains per gallon	0·0084	0·0075	0·0138	0·0083	0·0074	0·0083	0·0068	0·0082	0·0087	0·0083
Ratio of free										
To albuminoid ammonia	1 to 1·6	1 to 1	1 to 1·22	1·46 to 1	1·3 to 1	1·1 to 1	1·1 to 1	1·3 to 1	1·3 to 1	1 to 1·08
Oxygen required to oxidize.										
Parts per 100,000	0·20	0·13	0·37	0·30	0·18	0·19	0·18	0·18	0·16	0·16
Grains per gallon	0·116	0·105	0·21	0·175	0·105	0·111	0·105	0·105	0·093	0·093
Nitrites.										
Parts per 100,000	0·005	0·0066	0·005	0·0066	0·0083	0·0083	0·01	0·0083	0·005	0·005
Grains per gallon	0·0029	0·0038	0·0029	0·0038	0·0048	0·0048	0·0058	0·0048	0·0029	0·0029
Nitrates.										
Parts per 100,000	0·323	0·355	0·369	0·355	0·364	0·36	0·374	0·364	0·439	0·43
Grains per gallon	0·188	0·207	0·215	0·207	0·212	0·210	0·218	0·212	0·256	0·251
Chlorine.										
Parts per 100,000	0·45	0·525	0·65	0·70	0·65	0·65	0·65	0·60	0·45	0·475
Grains per gallon	0·262	0·306	0·379	0·408	0·379	0·379	0·379	0·35	0·262	0·277
Total solids.										
Parts per 100,000	18·00	18·00	18·50	18·50	18·50	18·50	19·00	18·50	18·50	18·50
Grains per gallon	10·49	10·49	10·79	10·79	10·79	10·79	11·08	10·79	10·79	10·79
Dissolved oxygen										
In one liter	.....	.....	.....	.....	4·41 cubic centimeters.	5·13 cubic centimeters.	4·34 cubic centimeters.	5·18 cubic centimeters.	5·32 cubic centimeters.	5·64 cubic centimeters.
Acidity.										
Calculated as sulphuric acid	0·00018 parts per 100,000	0·00002 parts per 100,000	0·00007 parts per 100,000							
Alkalinity.										
In terms of sodium hydrate	.....	.....	.....	*0·00003 parts per 100,000						

* The samples collected January 19 and 20, had a neutral reaction.

3. INTERPRETATION OF DATA OBTAINED BY ANALYSIS AND EXPERIMENT.

During the entire course of this investigation, one great object was left prominently in mind, and the character of the analytical operations performed was governed entirely by considerations as to what manner of analyses and experiments would throw most light upon the the question: Why at a certain particular period did the water in the Schuylkill river suddenly become unpalatable? My present object is to discuss seriatim the various classes of analytical results obtained, and to endeavor to present judicially and fairly the answers which these various classes of data severally give to the above questions.

1. THE FREE AND ALBUMINOID AMMONIA.

The information supplied by these determinations, bears especially upon the three following points:

1. The amount of Free Ammonia, which is the measure principally of that portion of the decomposable nitrogenous matter that has already undergone decomposition, is very much greater in all the samples (0·009 parts in 100,000 to 0·02075 parts) than in the water of the Schuylkill river, June 24th, 1881 (0·001 part).

2. The Albuminoid Ammonia, which is the measure of that portion of the nitrogenous matter as yet undecomposed, but capable of undergoing decomposition, is large (0·01175 parts to 0·02375 parts). At the same time it is not so great in any of the samples, with the exception of the one taken from the Fairmount Forebay, January 9th, as it was in the Schuylkill water, June 24th, 1881. Yet this last, as compared with the water supplies of the principal cities in the United States at that date, was ranked as third in order of purity. In other words, the absolute amount of decomposable organic matter in the Schuylkill water, at the time when it acquired its nauseous taste and smell, was not so great as at a season when it was palatable. This statement is borne out by the other analytical data, and I think it can be justly inferred that the unpalatability was due not to an unusually large amount of the polluting materials, which exist at all times to a greater or less extent in the waters of the Schuylkill, but to some peculiarity either in their character or in the nature of their decomposition.

3. There is no evidence, as will be seen farther on, of any peculi-

arity in the organic matter itself, which would account for the taste and smell. There is, however, a very striking peculiarity in the nature of its decomposition. By reference to the table of analyses, it will be seen that the *Free Ammonia exceeds the Albuminoid in almost every sample analysed.* That this is anomalous, will be seen by comparing these analyses with those of the various city waters in the United States, June and July, 1881.*

	Free Ammonia. Parts per 100,000.	Albuminoid Ammonia. Parts per 100,000.	Ratio.
Philadelphia	0.001	0.018	1 : 18
Boston	0.013	0.061	1 : 5
New York	0.0027	0.027	1 : 10
Brooklyn	0.00075	0.00825	1 : 9

This point is still more strikingly brought out by the series of analyses made upon 34 samples of water collected at various points along the Passaic river, from a point above the interruption of the sewage and factory refuse of Paterson, to a point below the lowest sewers of Newark, a distance of 20 miles.

These samples were all collected upon the same day, September 6th, 1881, when owing to three months of drought no water had gone over the dams for many weeks, and the water had become very offensive. But in the eleven samples in which the free ammonia exceeded the albuminoid ammonia, this excess was directly connected with the sewage of manufacturing towns and with the refuse of cotton, paper and woolen mills, and of dye-houses, thrown into the stream at points in proximity to those whence these samples were collected. Above these factories, and at a point 16 miles below them, near the intake of the Jersey City Pumping Station, there was little or no free ammonia in the stream. The differences are made plain in the following table:—

	Free Ammonia. Parts per 100,000.	Albuminoid Ammonia. Parts per 100,000.	Ratio.
Above Paterson	0·00	0·021	Infinite.
Below Paterson	0·2075	0·225	1 to 1
Below Passaic tail-race	0·0745	0·0215	3·5 to 1
16 miles below Paterson	0·0005	0·026	1 to 52

*For complete list, see *Jour. Amer. Chem. Soc.*

Since this time, during a period of eighteen months, there has never been an occasion when the water taken monthly at the Jersey City intake contained as much free ammonia as albuminoid ammonia. This remark is true of seasons when the flow of water being at its minimum and polluting agencies at their maximum efficiency; the water supply was otherwise adversely reported upon.

In 36 analyses of waters of the Brandywine river and certain other streams proposed as new sources of water supply, which I made in the year 1882, at the request of the City Councils of Wilmington, Del., there was not a single instance in which the free exceeded the albuminoid ammonia. The ratio varied between 1 to 43 at Rockland dam, which is the point I recommended as the best from which to draw the future water supply, to 1 : 1.2 at certain spots when the influence of trade and other pollutions became manifest.

Without detailing here the results of examination of many other streams, it is sufficient to say that the ratio of the free ammonia to the albuminoid ammonia usually varies in river waters from 1 : 2 to 1 : 10, and even higher amounts for the albuminoid ammonia. If the free ammonia exceeds, it can ordinarily be assumed that a very large amount of the *products* of decomposition derived from trade and sewage pollution are present. In such cases the absolute amount of albuminoid ammonia is also very considerable. But, as previously stated, the absolute amount of albuminoid ammonia in the Schuylkill river at the time when the non-potability occurred, though much exceeding the "limit of purity" in a river water whose free ammonia is at the same time very high, was not extraordinarily great. For this reason the excess of free ammonia indicates not only the fact of pre-existent trade and sewage contamination, but taken in connection with the other facts developed during the course of this investigation, has a special significance which will be explained farther on.

OXYGEN REQUIRED TO OXIDIZE THE ORGANIC MATTERS.

That the nauseous taste and smell was not due to the mere fact of an excess of decomposable organic matters is rendered still more probable by the small percentage of oxygen required to effect this oxidation. It is little more than one-fourth the amount of oxygen required on June 24, 1881, when the water was excellent, both in taste and smell.

NITRITES AND NITRATES.

A peculiarity of the same description exists with regard to the ratio between the nitrites and nitrates as between the free and albuminoid ammonia, in that the former is present in large amounts in all the samples. The origin of the nitrites is twofold. They are due either to direct oxidation of the nitrogenous organic matter—this oxidation stopping short of its proper final stage, which is the complete conversion into the form of nitrates—or they are formed at the expense of the nitrates already existing, by their reduction in presence of excess of readily oxidizable organic matters.

Both modes of origin are indicated in the present instance, and both point to the same conclusion that there was an excess of organic matter beyond what could be oxidized under the existing circumstances.

On account of the extreme instability of the nitrites, they are usually converted almost entirely into the form of nitrates, and leave behind but little more than a trace of their presence. Their large percentages in these samples indicate, therefore, a retarded or arrested oxidation.

COMPLETE ANALYSES OF THE SCHUYLKILL WATER SUPPLY.

	Pottstown, January 10, 1883.		Fairmount, January 9, 1883.	
	No. 226.		No. 228.	
	Parts per 100,000.	Grains per gallon.	Parts per 100,000.	Grains per gallon.
Lime	3·598	2·095	3·520	2·053
Magnesia	1·515	0·884	1·034	0·603
Oxide of iron and alumina	0·134	0·077	0·264	0·154
Ammonium hydrate	0·019	0·012	0·040	0·023
Sodium chloride	0·590	0·344	0·930	0·542
Potassium chloride	0·180	0·105	0·187	0·109
Carbonic anhydride	0·780	0·455	0·710	0·404
Sulphuric anhydride	2·300	1·341	1·890	1·1
Silicic anhydride, and insoluble matters	1·289	0·752	1·950	1·137
Phosphoric anhydride	trace.		trace.	
Nitrous anhydride	0·008	0·005	0·008	0·005
Nitric anhydride	0·560	0·327	0·640	0·373
Inorganic matter	10·968	6·393	11·173	6·523
Total solid matter in solution	18·000	10·497	18·500	10·789
Organic matter (by difference)	7·032	4·104	7·327	4·266

CHLORINE.

The percentage of chlorine is somewhat high, and is relatively greater at Fairmount than it is at Roxborough, Norristown, and Pottstown. It is also higher than at other seasons when the character of the water has been better. All these facts are in accord with what would occur from sewage contamination, but do not aid us materially in discovering the cause of the January disorder.

TOTAL SOLIDS AND COMPLETE QUANTITIVE ANALYSIS.

The determinations of the amounts of saline bodies and organic compounds which exist in the water in a state of solution, and are left behind as a residue upon evaporating 100 cc. of the water to dryness and drying at 110°C., throw no light upon the present question. They merely show that the amount of substances in solution is greater by $\frac{1}{2}$ part in 100,000 at Fairmount than in the upper portion of the stream. It is not unusual to strongly heat these residues and set down their losses in weight as organic and volatile matters. For a variety of reasons the results thus obtained are so inaccurate as frequently to mislead, and for this reason have not been made use of in the present difficult inquiry. Instead, I have given below the results of two complete analyses of the mineral constituents. The difference between the sum total of these mineral constituents and the total solids in solution is regarded as representing the organic matter approximately. Unfortunately, no accurate determination of the total amounts of the very complex and very various organic bodies present in natural waters is possible in the present state of our knowledge. The results here given represent the closest approximation to the truth which I was able to arrive at, and, I think, are sufficiently correct to serve as a trustworthy basis for reasoning.

An examination of these complete analyses shows that the lime, magnesia, carbonic anhydride and sulphuric anhydride are all less at Fairmount than at Pottstown. They have probably been precipitated out of solution on account of the relative insolubility of the carbonates and sulphates of lime and of the carbonate of magnesia. The increased amounts of sodium and potassium chloride, of ammonium carbonate, of nitrous and nitric anhydrides, would all be in accordance with the fact of contaminating with sewage and trade pollution in the lower portion of the stream, as would also be the slightly increased amount of organic matter, shown in the analysis of the Fairmount, as compared with the Pottstown water. But this increase is not so important

as the fact that the organic matter, even in the upper portions of the stream, was very considerable, amounting to over 7 parts in 100,000. And what is important for us to note here is that to this considerable burden of organic matter, which might or might not be of unwholesome character, was added to the customary increment of pollution from sewers and factories in the lower portion of the stream. As to the origin of this organic matter, the following letter is important:

"*Reading, January 11, 1883.*

"DR. WM. H. MCFADDEN,
Chief Engineer Water Department.

"DEAR SIR:—Referring to your favor of 9th inst., asking for the facts regarding the present condition of the Schuylkill, I have to say in reply that all the dams of the Navigation Company above the Blue mountains, from No. 7, at Schuylkill Haven, to No. 16 (Blue mountain), were drawn between December 18th and 23d, and they remain so drawn at this time. The water was let out from each dam by means of sluices in them provided for that purpose, and canals connected with them were emptied at the same time. All these dams had ice upon them from 4 to 6 inches in thickness when drawn. Between the Blue Mountain and dam No. 22, North Reading, none of the dams have been drawn; they are now and have been covered with ice about 6 inches thick, since the middle of December.

"Ice began to be formed soon after the 26th of November, and the obstruction was complete as early as the 4th of December.

"Dams Nos. 23 and 24 (Poplar Neck and Lewis) below Reading, the latter known as the big dam, were drawn respectively on the 29th of December and January 1st, and there was very little ice on either at these times. The dams between No. 24 and Fairmount, six in number, have not been drawn, but have all been more or less obstructed by ice since December 4th, varying in thickness from 2 to 5 inches.

"The flow of the river has been much below the usual minimum, both during the fall and ever since winter set in, and the rain-fall in the valley has not been more than half of what is usual during the same period.

"The dams not drawn, nevertheless, have all continued to run over, but with a flow scarcely more than that of the low water of summer, whilst the dams below 'Lewis,' or 'big dam,' have not been drawn, all the canal levels which they supplied have been emptied. One series

of these levels, 22 miles in length, supplied from the 'big dam,' was drawn out about the 16th of December, and furnish a very large body of pure water to the river. Another, the five-miles level, opposite Royers' Ford, was drawn on the 23d of December. This level receives the drainage from the wood-paper works at Spring City. The level opposite Phœnixville, 4 miles in length, was drawn out on the 22d of December.

"Yours, very respectfully,

"JAS. F. SMITH."

In commenting upon this letter, the Chief Engineer emphasizes the three most important facts thereby elicited.

1. The small rainfall in October, November, and December, and the very small flow of the river.

2. The emptying of the Schuylkill canals and eleven dams thereon.

3. The complete covering of ice on the Schuylkill river, the canals, dams, and tributaries.

Whilst the first two points throw much light upon the origin of the large percentage of organic matter, they cast but little upon its character,—whether it was necessarily wholesome or unwholesome, whether it imparted the bad taste and smell. With reference to possible unwholesomeness we must remember that the organic matter which contains nitrogen is the portion most important to study. The amounts of albuminoid ammonia (0·0145 parts to 0·02375 parts) indicate that this constitutes only a small fraction. If we assume, along with the author of the albuminoid process, that the nitrogenous organic matter may be estimated as ten times greater in amount than the albuminoid ammonia, this fraction would not exceed 0·2375 parts in a total of 7 parts in a thousand. I am aware that the experiments by which this proportion was established have recently been repeated by Preusse and Tiemann,* mostly upon artificial organic compounds, far more stable in their nature than the putrefiable substances, the estimation of which especially concerns us in hygienic investigations of water supplies. These experiments, which were less extended and elaborate than those of Wanklyn, merely confirmed his results. Their conclusion that the process gives us no indication of the absolute quantity of nitrogenous organic substances, no more invalidates the process than the analogous

* Ber. der deutsch. chem. Gesell, xii, 1906.

results they found by the permanganate method of estimating organic matter militates it. And yet, whilst they obtained less decisive results by permanganate process, urea for instance not being at all oxidized, they placed great stress upon the indications afforded by the permanganate method. It may, likewise, be inferred from experiments identical in character with those by which Wanklyn assumed that the disintegrating animal refuse in water might be fairly estimated as ten times its albuminoid ammonia, that its total organic matter is about 20 times greater than the oxygen in the permanganate required to effect its oxidation. We have already seen that the organic matter in the Pottstown sample amounted in all to 7·032 parts per 100,000, and in that from Fairmount to 7·327 parts. Estimated by the above rule, the relative amounts of organic matter would have been 4·0 parts in 100,000 and 7·4 parts respectively. Fallacies inherent in the permanganate method, and inductions based thereon, make this lack of concordance probable. Nevertheless, it may safely be assumed that of the total of 7 parts of organic matter per 100,000, not more than 0·5 parts were readily decomposable organic substances, or were derived from sewage.

DISSOLVED OXYGEN.

I did not attempt to determine the oxygen in the samples collected when the water was at its worst phase, the samples having stood several days. But the determinations made upon the fresh samples, collected by myself and analyzed with least possible delay, bring into prominence certain points which bear directly upon the vital question of the present investigation. These relate, in the first place, to the smaller volumes of oxygen in the samples as compared with the volumes which should have been present in case the water had been completely aerated, and, in the second place, to the different volumes of oxygen in the pairs of samples collected at the same spot, but the one from the top, the other from the bottom of the stream.

The largest volume in any of the six (Nos. 230, 235) is that contained in No. 234, the sample taken from the bottom of the stream at Roxborough basin. It is 5·64 cubic centimeters in a liter of the water measured at 15° C. But this amount is nearly a cubic centimeter less than I found to be present in a liter of distilled water, artificially aerated by a current of filtered air, and which contained 6·51 cc. of oxygen.

A liter of No. 230, similarly aerated, had its volume of oxygen raised from 4·41 cc. to 6·3 cc., and another liter of No. 230, aerated by mere exposure for a number of days, in flasks plugged with cotton wool, to the atmosphere, had its volume of oxygen increased to 6·2 cc.

These aerated samples were afterwards subjected to sun-light, in the presence of nitrate of silver, when it was found that the amount of oxygen required to oxidize the organic matter in that portion of No. 230, which had been aerated by a constant current of air, had been reduced from 0·218 part in 100,000 to 0·044 part. The organic matter in the other sample of No. 230, aerated merely by exposure to the atmosphere, had been reduced from 0·218 part to 0·103 part.

These facts, it appears to me, are conclusive both as to the lack of oxygen in the samples collected and the effect of oxygen on that small portion of the total organic matter which was susceptible of easy decomposition or putrefaction and of as easy oxidation.

Very noteworthy, also, are the differences between the volume of oxygen contained in the corresponding series of samples taken from the top and bottom of the stream, the oxygen being uniformly greater in the bottom water. The differences are real, whatsoever opinion may be obtained as to their cause. The results being surprising, the determinations were oft repeated, but the closely concordant figures were decisive as to the fact of the larger volumes of oxygen being present in the samples taken from the bottom. It is also noteworthy, that in the Roxborough samples the volumes of oxygen are greater in both top and bottom sample, than in the corresponding samples at Spring Garden and Fairmount.

The bearing of the above facts upon the great question, kept constantly in view in this investigation, can be best appreciated in the light of the results obtained by certain European chemists, more especially by M. Dumas, his examinations beginning with the river Seine, above Paris, and extending to Rouen, about one hundred miles below. He found that the river water contained at the first named point, 9 cubic centimeters in a liter; at Point d'Ivry, near the upper border of the city, 8 cc.; at Point de la Tournelle, near the middle of the city, 8 cc.; below the city, and above the outlets of main sewers, 6 cc.; at outlet of lower main intercepting sewer, 2 cc.; at Epinay, a point located below all sewers, 1 cc.; at Poissy, 17 miles below Epinay, 6 cc.; at Vernon, 27 miles below Poissy, 9·5 cc., and at Rouen, 35

miles still lower down, even more than in the waters above Paris, or 10·5 cubic centimeters in a liter.*

Still more striking results were obtained by M. Gerardin, who extended the inquiry so as to embrace the three following topics:

1. A determination of the amount of oxygen held in solution.
2. An observation of green plants and aquatic mollusks.
3. A microscopic examination of algæ and infusoria.

It is claimed that the results obtained by these three methods were identical, and that, where the water was clear, with abundance of fish, water-cress, etc., it contained a correspondingly large amount of oxygen; while in places where the dissolved oxygen was small, fish and the higher types of aquatic plants were wanting, and certain low forms of vegetable growth had taken their place. The river Vesle, in France, from Rheims to Braisne, was taken as the field of observation. It was studied over a distance of 37½ miles, during which it received the sewage of one large town (that of Rheims), amounting to 4,180,000 gallons, and other impurities. Above Rheims the water, which was clear, wholesome and with abundance of fish, charas, water-cress, iris, etc., contained 1 cubic inch of oxygen in 100 cubic inches.

In passing through a suburb above Rheims, the Vesle received the refuse of some dye works, which colored the water; and in place of the fish and water-cress, Sparganium simplex made its appearance. At a point where the water had received the contents of the five principal sewers of Rheims, the water was thoroughly polluted, and contained but ·05 cubic inch of oxygen in 100 cubic inches. Two species of algæ, the Beggiatoa alba, and the Oscillaria natans were developed largely, the latter to such an extent, that the whole surface of the sluggish water was covered with a thick, blackish coat. This coat was seemingly so solid, that animals, and even men, ventured upon it, mistaking it for terra firma. Above the mill at Macan, when the oxygen had increased to 0·74 cubic inch, the two varieties of algæ mentioned above, had disappeared, and the bed of the Vesle was covered with a long, whitish algæ, called Hypheothrix. At Compensé mill, the oxygen had increased to 0·8 cubic inch, the hypheothrix had almost completely disappeared, and the Sparganium simplex was again abundant. Below

* It is probable that these figures for dissolved oxygen, which were obtained by M. Schutzenberger's process, are all too high. But the ratio between their relative values is probably approximately correct, and is sufficient to establish the point under consideration.

this point the amount of oxygen increased, and with it a corresponding change took place in the vegetation, until, at Braisne, the water contained 1 cubic inch of oxygen per 100 cubic inches, all traces of pollution had disappeared, and fish and water-cress flourished. From this it would appear that a properly aerated and pure water showed, when polluted, the amount of pollution by a corresponding diminution of oxygen, by the appearance of Sparganium simplex, Spirogyra, Hypheothrix, Beggiatoa and Oscillaria, and progressive improvement by a corresponding increase of oxygen, and the appearance of these plants in reverse order.

ACIDITY AND ALKALINITY.

Potable water should have a neutral reaction. This was not the case with the Schuylkill water. It was decidedly acid in its upper portion (January 10), less so in its lower portion (January 9), and in the interval between January 9 to January 13, its reaction changed from acid to alkaline. Six days later when from all accounts its taste and smell had much improved, it was neutral. This change of reaction could not take place without influencing the constitution of the water, since it would tend to precipitate various bodies held in solution. It is difficult to speak more precisely on these points, since the determination of the acidity and alkalinity of potable waters has been hitherto generally overlooked, and from the lack of data it can only be said that this change of reaction was associated with, and favorable to a rehabilitation of the potability of the water supply.* What we must emphasize is that the water coming down the stream after the 9th of January was of a very different character from that of the water before this date. The acid water had all gone by the Fairmount pool before the 13th of January, since at the latter date the water was no longer acid but alkaline. In other words, the influence of the limestone waters flowing into the Schuylkill was more pronounced on the 13th of January than the influence of the acid waters emptying into the stream in its upper portion at points above where the limestone-bearing tributaries join the Schuylkill. This being the case, the origin of the bad taste and smell is likewise connected to a certain extent with the upper portion of the river. It was due in part at least to other causes than the irruption of sewage and factory waste into the Roxborough, Spring Garden, and Fairmount pools.

* See a valuable contribution to this subject by Dr. C. M. Cresson and Mr. H. W. Mitchell in pamphlet "Results of Examination of Water from the River Schuylkill, Phila., 1875."

COLOR, TASTE, SMELL, AND HYGIENIC EFFECTS.

Though none of the samples were colorless as compared with distilled water, the estimation of the precise tinge was of no importance in this investigation.

The taste and smell are difficult to define since subjective impressions cannot well be described unless they originate from familiar substances. Many ascribed to the water the odor of carbolic acid; others that of chloride of lime; most likened it to something of an oily, greasy or fatty character.

A large quantity of the water was tested for carbolic acid, but not a trace was found, nor any other indication of the presence of bodies derived from coal-tar or gas works refuse.

No metallic poisons were present, nor any substance like chloride of lime.

Letters addressed to eminent physicians in Philadelphia, elicited the uniform response that no connection could be established between the character of the Schuylkill water supply in the month of January and any case of disease within their practice. The following table compiled by order of W. H. Ford, M. D., Chairman of the Sanitary Committee, Philadelphia Board of Health, may be studied by any who feel able to draw satisfactory conclusions therefrom.

Deaths from some of the principal zymotic diseases in Philadelphia for sixteen months, commencing with November, 1881.

DISEASES.	MONTHS.															
	November, 1881.	December, 1881.	January, 1882.	February, 1882.	March, 1882.	April, 1882.	May, 1882.	June, 1882.	July, 1882.	August, 1882.	September, 1882.	October, 1882.	November, 1882.	December, 1882.	January, 1883.	February, 1883.*
Cerebro-spinal meningitis	3	10	3	2	10	6	7	4	4	7	4	2	.....	2	5	3
Cholera morbus	1	1	.....	1	1	.....	1	1	18	33	6	1	.....	.....	.....	1
Diarrhœa	10	11	6	12	21	14	12	3	25	35	13	10	9	8	11	8
Diphtheria	75	64	83	55	48	41	66	55	55	53	95	112	137	132	127	51
Dysentery	1	7	2	7	5	2	2	6	10	16	11	7	3	1	2	2
Typhoid fever	85	67	60	56	5	79	76	57	28	52	49	52	33	53	57	28
Inflam'n of stomach and bowels	25	35	42	30	46	32	35	38	47	88	54	47	30	32	30	23

* February, only three weeks.

MICROSCOPIC EXAMINATION.

In pursuance of the methods of Koch, and after a plan suggested by Dr. R. Angus Smith, 50 cubic centimeters of each sample were added to 25 cc. of a 2½ per cent. solution of gelatin in distilled water, 5 mgrm. of sodium phosphate added to each, and the flasks containing the solutions allowed to stand in a warm place. After five days, liquefaction began on the upper surface of the jelly-like contents of one flask, that containing No. 230, and subsequently one after another the remaining flasks also became filled with micrococci and bacteria. This mode of inquiry afforded no aid in the study of the present problem.

THE GRAPHIC CHART.

I have appended to this report a graphic representation, which puts in clearer light the obtained results. It presents sanitary analyses of ten samples collected January, 1883, and, for comparison, that collected June 24, 1881. Also, complete analyses of the inorganic matter in the Pottstown water (January 10) and the Fairmount water (January 9). Constituents of the same order of magnitude are represented on the same scale; the upper third of the map being given to free and albuminoid ammonia and nitrites; the middle to required oxygen, nitrates and chlorine; the lower third to dissolved oxygen. In the last, the scale represents volumes or parts of cubic centimeters of oxygen per liter of water.

Inspection shows that the *free ammonia* was least at Pottstown. The curve rises and crosses above that for the albuminoid ammonia at Fairmount, January 13, and does not fall below it until it represents the bottom water in Roxborough pool, January 20. For the second series of samples, the maximum elevation of the free ammonia curve is found at Spring Garden basin.

Albuminoid ammonia rises to its maximum at Fairmount forebay January 9.

For reasons previously explained, the curve of the *required oxygen* does not repay study, which represents the results obtained by the permanganate process.

Nitrites show maxima at Fairmount and Spring Garden.

Nitrates are greatest at Roxborough, and what is striking, at the same point the curve for the nitrites falls, as though the reduction due to the incursion of fresh sewage was less at that point.

Chlorine rises steadily with the flow down the river.

Dissolved oxygen rises and falls at top and bottom of the river, but shows a relative rise in both for the Roxborough samples.

4. LIGHT THROWN BY THE RESULTS UPON THE ORIGIN OF THE JANUARY TASTE AND SMELL.

It has been stated previously that the mere fact of sewage contamination is insufficient to explain the taste and smell. The anomalous accumulation of the products of decomposition; the free ammonia exceeding the albuminoid; the reduction of the nitrates to the form of nitrites, instead of remaining in the former form; the deficiency of oxygen in all samples as compared with the normal amount; the completer oxidation of the decomposable organic matter when the samples were allowed to absorb their full complement of oxygen—all of these facts are significant of a different explanation. This is, that the taste and smell were connected with a state of partial and arrested oxidation, and of incomplete aeration. Moreover, the bodies which gave rise to the unpalatability were products of decomposition of organic substances contained in the water, or deposited at the bottom of the stream, the nature of these bodies being determined by the fact of their being formed by processes of putrefactive decay out of contact with oxygen. Under these conditions it is well known that hydrocarbons belonging to several isologous and homologous series are formed, the lower members of which are gases; the members of greater molecular condensation, oils; and the highest, solids. The most familiar instance is that of marsh gas, which is being constantly liberated from vegetable matter decomposed at the bottom of ponds out of contact with air. Another familiar instance is that of the oil frequently seen floating on the surface in similar localities, and originated in the same way, but differing from marsh gas by greater condensation of molecular structure. When this decomposition occurs under water containing (like that of the Schuylkill river) sulphates, these sulphates undergo reduction, and sulphuretted hydrogen and sulphuretted hydrocarbon compounds are liable to be found, all of which compounds are foul-smelling and ill-tasting.

Whilst the opportunities afforded me for investigation (the inquiry having been put into my charge only after the crisis of the disorder had passed and the great body of the unpotable water had gone by Fairmount pool) were too limited to permit me to offer the above,

except as a probable explanation, yet I trust I may not be thought lacking in scientific caution so to do. The explanation accounts for the combustible gases found beneath the covering of ice, and which, according to the testimony of Professor Houston, were in sufficiently large percentage to be set fire to.*

It accounts for the volatile oily bodies so generally complained of, so persistent as to communicate their taste to the food cooked in them, and to diffuse through the house and kitchen their disagreeable odor. It explains their connection between the acid water of the upper Schuylkill and the contemporaneous accession of unpalatability, this upper water, already charged with products of decomposition accumulated in ice-covered dams and canals, being further deteriorated by bodies peculiarly susceptible to putrefactive decay, and to an extent exceeding the maximum limit of permissible sewage contamination in the lower portions of the stream.

5. QUESTIONS RELATING TO THE DETERIORATION OF THE WATER SUPPLY.

Such a conjunction of untoward circumstances as resulted in this January cachexy of the Schuylkill water supply seldom occurs. For this reason its consideration may be dismissed until opportunity is afforded for a complete investigation of similar disorders and for discovery of appropriate remedies.

At present it is more important to note that the water in the Fair-

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* The following letter, received long subsequent to the publication of the original report, gives an interesting account of these remarkable phenomena:

PHILADELPHIA, *June* 6, 1883.

DR. ALBERT R. LEEDS,

DEAR SIR:—In reply to your letter of inquiry, we write to say that you have been correctly informed concerning the inflammable gas. It was first shown to us by Mr. Alan Gentry, of Germantown, and we have subsequently repeated the experiment on several occasions. The gas was contained in white hollows under the surface of the ice, and whenever we noticed one of these, we punctured it with a penknife and applied a match. The gas took fire readily, and burned with a blue flame, yellow at the top. A noteworthy fact in connection is that the gas was found only on the western bank of the river, where the water is shallow, and the bottom of deep black mud. The flames were ordinarily about six inches high, but once we obtained one fully a yard high. They burned usually for about two minutes.

W. A. MACKIE,
E. E. MATHER.

mount and Spring Garden pools often deteriorates to a point below the maximum limit of admissible impurity. And whilst the evidence afforded by the single analysis of the water in the Roxborough pool, which was only incidentally made in the course of this investigation, is much in favor of the latter, yet the relative character of the Roxborough water can only be established with certainty by more extended research.

THE CHEMISTRY OF THE PLANTÉ AND FAURE ACCUMULATORS.*

By J. H. GLADSTONE and ALFRED TRIBE.

(Continued from vol. cxv, page 68.)

PART V.—1. INFLUENCE OF STRENGTH OF ACID.

In the second part of this communication, see this *Journal*, vol. cxiv, p. 223, when treating of the charging of the cell, we pointed out that in the electrolysis of dilute sulphuric acid between lead electrodes, two totally different reactions might be obtained. The positive metal becomes thinly coated with lead sulphate when the current employed is of small density, but with lead peroxide when the density of the current is of greater magnitude. This latter action is, of course, what takes place in the ordinary formation of a Planté battery. The chemical change, therefore, which goes on at the positive electrode is to a certain extent dependent upon the strength of the current. It appeared also of both theoretical and practical interest to determine whether the chemical change was also influenced by the strength of the acid employed. Our experiments consisted in passing a current of uniform strength, about 1 ampere, between electrodes of lead, 12 square inches in size, in varying strengths of sulphuric acid, and estimating in each case the amount of oxygen fixed by the positive electrode. We determined this for successive five minutes of time, and as such actions are not always very uniform, we made in each instance more than one experiment. The results are given in the following table:

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* From London *Nature*.

Strength of acid.	Expt.	Percentage of oxygen fixed.				
		First 5 mins.	Second 5 mins.	Third 5 mins.	Fourth 5 mins.	Total.
1 to 5.....	I.	38·1	28·6	28·6	33·3	128·6
	II.	39·5	30·2	25·6	30·2	125·5
1 to 10.....	I.	43·4	38·7	29·2	34	145·3
	II.	44·1	39·3	29·3	34·9	147·6
1 to 50.....	I.	48·3	39·6	35·3	22·4	145·6
	II.	46·2	43·9	23	30	143·1
	III.	54	40	35·3	35·5	165
1 to 100.....	I.	42	38·3	38·9	29·5	148·7
	II.	42·4	40	37·8	35·5	155·7
	III.	51·1	44·2	34·9	34·9	165·1
1 to 500.....	I.	46·6	32·6	27	27	132·6
	II.	46·4	27	27	18	118·4
1 to 1,000.....	I.	90·6	81·1	76·4	57·5	305·6
	II.	90·8	77	72·3	63·1	303·2

It appears from this that the strong sulphuric acid (1 to 5) is not quite so favorable to the action as the more dilute (1 to 10), but that between this latter proportion and 1 to 500 there is no great difference in the amount of oxygen fixed, and therefore of corrosion of the plate. The appearance of the plate in every instance indicated the formation of peroxide lead only. With sulphuric acid diluted with 1000 parts of water, the amount of oxygen fixed, and therefore of corrosion, was at least doubled, while the chemical action was very different. On parts of the electrode, streaks of a mixture apparently of the yellow and puce-colored oxides were seen. On other parts a white substance formed and was easily detached, falling in clouds into the liquid. Where this latter action took place, the plate was visibly the most corroded. This white substance gave on analysis SO_4, equivalent to 73·6 per cent. of lead sulphate, suggesting the idea that it was a basic sulphate of the composition $2PbSO_4,PbO$, which would require 73·1 per cent. As the peroxidation of the lead is required, and the corrosion

of the plate is to be avoided as much as possible, it is evident that this extremely dilute acid must be avoided. It has already been shown that if the sulphuric acid is entirely removed from solution, as sometimes happens in an accumulator, the lead is simply converted into the hydrated protoxide, and therefore corroded without any good effect.

2. FUNCTION OF HYDROGEN.

In the formation of a secondary cell, after the complete reduction of oxide or sulphate to metallic lead, bubbles of hydrogen gas are seen to escape from the lead plate. It has been assumed that a portion of this is occluded by the lead, or in some other way enters into association with it, and it has been supposed that this hydrogen compound may play an important part in the subsequent production of electromotive force. It therefore appeared desirable to obtain experimental evidence as to whether hydrogen is so absorbed. The process we adopted for this purpose was founded upon the observation of Graham that hydrogen associated with palladium reduced ferri- to ferro-cyanide of potassium, and that generally in the occluded condition the element was more active chemically. We had previously ascertained that hydrogen associated with other elements, as platinum, copper and carbon, was capable of reducing potassium chlorate to chloride. This method seemed to give trustworthy results, and therefore we applied it in this instance. As the result of several trials, however, we found that the amount of hydrogen associated with the reduced lead was almost inappreciable. Small as this quantity is, however, it is by no means impossible that it may be the cause of the exceedingly high electromotive force observed for the first few moments on joining up a completely formed cell immediately after its removal from the circuit of the charging current. This, however, may be due, as Planté imagined, to the gaseous hydrogen itself. The principal if not the only function of the hydrogen of the water or sulphuric acid is therefore that of reducing the lead compounds.

By a totally different process Prof. Frankland has very recently come to the same conclusion as ourselves in regard to the exceedingly small amount of occluded hydrogen.

3. EVOLUTION OF OXYGEN FROM THE PEROXIDE PLATE.

Planté noticed a small escape of gas from the negative plate of his cell immediately after its removal from the influence of the charging

current. This he attributed to a decomposition of water by means of local circuits between the peroxide and the subjacent lead plate in contact with it.

The explanation we gave in our first paper (this *Journal*, vol. cxiv, p. 219) of the local action which goes on at the negative plate does not account for the escape of any gas—either oxygen or hydrogen. We therefore thought it of interest to ascertain the nature, and if possible the origin of the gas noticed by Planté.

We found that the escape of gas from a Planté negative plate was very slight, and soon ceased; but we observed that it became much more pronounced when the temperature of the electrolytic liquid was raised. In order to get a sufficient quantity of the gas for examination, we prepared a negative plate according to the procedure of Faure, and then heated it in dilute acid, with an arrangement for collecting the gas as it was evolved. The amount of gas was still very small in comparison with that of the peroxide, but a sufficient quantity was collected to enable us to ascertain that it was oxygen. We next heated some of the electrolytic peroxide apart from the lead plate, and again noticed a similar evolution of gas, which was also found to be oxygen. This shows, therefore, that it was not a result of local action.

The gas has generally some odor of ozone, and on testing the dilute acid between the plates of a Planté cell we always found traces of something that bleached permanganate of potassium, and which might be either ozone or peroxide of hydrogen.

The origin of the gas noticed by Planté may be easily attributed to the oxygen which always passes off in quantity from the peroxide plate during the process of "formation." It is only necessary to suppose that some of this becomes condensed on the peroxide, and is gradually eliminated from it when the surrounding conditions are changed. But the matter is capable of another explanation. If peroxide of hydrogen be really formed in the liquid, it will exert its well-known influence on higher oxides, namely, that of reducing them and itself at the same time. As a matter of fact, if peroxide of lead is dropped into peroxide of hydrogen, oxygen is evolved.

4. TEMPERATURE AND LOCAL ACTION.

Planté has recently pointed out that an elevation of temperature facilitates the formation of his secondary cell (*Comptes Rendus*, August, 1882). The character of the chemical changes which took place at the

negative plate led us to think it exceedingly probable that this increase in the rate of formation arose from an augmentation in the amount of local action. Experiment showed such to be the case. Pairs of similar negative plates on Planté's model were allowed to remain in repose at 11°C. and 50°C. respectively, and the formation of the white sulphate was visibly more rapid at the higher than at the lower temperature. The same is also true with negative plates prepared by Faure's process. Thus we found that two similar plates kept in repose for an hour, the one at 11°C. and the other at 50°C., formed by local action 2·6 and 7·4 per cent. of lead sulphate respectively. On two other plates the proportions were 7·6 and 9·5 per cent. respectively. These observations of course by no means exclude the idea that an increase of temperature may facilitate the other chemical changes that take place in the formation of a lead and lead-oxide cell.

PEMBERTON'S METHOD FOR THE VOLUMETRIC ESTIMATION OF PHOSPHORIC ACID.

By Prof. G. C. Caldwell.

[From advance sheets of the Second Report of the Cornell University Experiment Station.]

Mr. H. Pemberton, Jr., has described (Journal Franklin Institute, March, 1882, p. 184) a new method for the volumetric determination of phosphoric acid, especially in fertilizers, by a standard solution of ammonium molybdate; he adds the precipitant to the suitably prepared solution of the fertilizers as long as a precipitate is produced, the final tests for further precipitation being made with small filtered portions of the solution under examination. He prepares his standard solution by dissolving 89·543 grams of ordinary ammonium molybdate in 1000 cc. of water; each cubic centimetre of such a solution precipitates 3 milligrammes of phosphoric anhydride. The solution of the phosphate is freed from silica and organic matter, then made up with as little excess of acid as possible, and just neutralized with ammonia; 2 cc. of nitric acid (sp. gr. at least 1·4) and 10 grams of ammonium nitrate are added, and the solution is heated to 60° Cent., or above, preparatory to adding the molybdate; it is necessary to apply the filtration test only towards the end of the operation, when it becomes difficult to discern the effect of further addition of the molyb-

date, even in the supernatant liquid that has been somewhat clarified by standing for a minute or two.

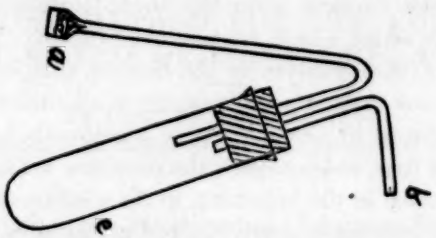
The frequent need of the determination of phosphoric acid in fertilizers, the absence of any thoroughly reliable volumetric method for this determination, and the length of time required for the gravimetric method, when the acid must first be precipitated by molybdate, all unite to give special interest to such a method as this, in which the molybdate itself, giving so sharp a reaction for the acid, is used directly in the standard solution.

With one modification in the preparation of the standard solution, and another and more important one in the mode of filtering out the test-portions, the process has satisfactorily sustained a series of careful tests in the laboratory of the University. As to the preparation of the standard solution, Pemberton found that the ordinary ammonium molybdate of the druggists was sufficiently pure to give the standard directly; in order to clear away the slight turbidity which the salt usually leaves he added a little ammonium hydrate. It was observed by Dr. Newbury, then analyst to the Station, that in the case of the molybdate supplied to us, a notable degree of alkalinity was communicated to the solution by the quantity of ammonium hydrate required to clarify it, and that, consequently, the necessary acidity produced by the nitric acid added in the beginning to the solution of the phosphate, became so much reduced towards the end of the titration after a considerable quantity of the molybdate had been added, that the results were unsatisfactory; in some instances the solution even became slightly alkaline before the precipitation was completed. In order to avoid this difficulty the molybdate solution was filtered after the addition of a few drops of ammonia at the most, and then standardized by a solution of phosphorus salt in which the phosphoric acid had been carefully determined by molybdate and magnesia mixture in the usual manner.

As to the manner of taking out the final test-portions, even when this operation is performed as described by Mr. Pemberton, this method of determining phosphoric acid is unquestionably more expeditious than the usual precipitation method; but the necessity of filtering out each test-portion in the ordinary way through a filter has its evident objections. For this filtration we have used in this laboratory with entire success a little contrivance that was applied with like success some years since in the determination of sulphuric acid by a

standard solution of barium chloride, in a modification of Wildenstein's method as described by Fresenius. The apparatus, as still further improved, more recently, is shown in the adjoining figure. The internal diameters of the bent tubes *a* and *b* should be about one millimetre, and that of the mouth at *a* about five millimetres. The length of the arm *a* may of course be adapted to the depth of the beaker used. To prepare the filter for use a small perforated platinum cone, or a tangle of fine platinum wire, is crowded into the throat of *a* so that it will not fall out when the mouth of the tube is downwards; suction is applied at *b* by a rubber tube to the mouth or connected with the exhaust; *a* is dipped for an instant only in a very thin asbestos pulp, and the suction is continued while the filter is then plunged into water and till the wash water comes through clear; it is then ready for use.

In using it, the only precaution to be observed, and that is an essential one, is never to plunge the filter into a liquid unless suction is



applied to it, and to continue the suction as long as it remains in the liquid; otherwise the asbestos and cone may fall out of the tube. The filter being thus withdrawn, the liquid contained in it and in the tube *a* is drawn over into the test-tube; two or three drops of the molybdate solution are run directly into *c* to test for complete precipitation, the rubber cork and its two tubes being meanwhile conveniently hung on a hooked wire attached to the lamp stand; the test being completed by heating the contents of the test-tube just up to boiling, the liquid is poured back into the beaker and the tube is rinsed also into the beaker with a very little hot water; when the next test-portion is to be filtered out the first filtrate drawn over is poured back into the beaker, thus using this portion to wash out of the filter and tube the last portions of liquid adhering to it from the previous filtration. The final reaction is so sharp, that, towards the end of the titration, we have found it sufficient to add to the solution in the beaker only the few drops of molybdate added to the test-portion.

In the following table the results are given of our tests of this method of determining phosphoric acid. The solutions of the phosphates analyzed and of the molybdate were prepared by Dr. Newbury, and the gravimetric determinations by the ordinary molybdate method were also made by him. The several volumetric tests following were made, using these solutions, by Dr. Newbury (I), Dr. S. M. Babcock (II), then instructor in the chemical department, and myself (III), and in every case in entire ignorance of the results obtained by the others. Mr. Furry (IV), at present the analyst of the station, prepared his own solutions throughout.

Station Number.	I.		II.	III.	IV.
	Grav.	Vol.	Vol.	Vol.	Vol.
235	.....	13·14	13·27	.....	13·13
		13·14	.....	.....	13·18
					13·15
					13·12
259	12·47	12·33	12·39	12·44	12·34
	12·41	12·33	.....	12·45	
				12·47	
261	9·15	9·26	9·08	9·20	9·34
	9·21	9·20	.....	9·05	9·37
				8·97	
				8·97	
272	13·09	13·57	13·75	13·82	
	13·82	13·57	.....	13·81	
		13·09	.....	13·72	
		13·09			
		13·63			

Most of these results speak well in favor of the method, and it appears to be capable of greater accuracy than is here indicated; for in most cases, and apparently in accordance with Mr. Pemberton's directions, larger quantities of the molybdate were added between each test towards the close of the titration than were necessary; these addi-

tions should be made by 0·1 cc. at the most. But the results obtained in the case of No. 261 do not agree closely enough hardly even for technical work; there seemed to be some source of error there which may be revealed by a further examination of the phosphate.*

I am permitted by Dr. S. M. Babcock, now chemist of New York State Experiment Station, to mention in this connection his good success in determining reverted acid by precipitation with magnesia mixture from the ammonium citrate solution, according to Johnson's method, solution of the precipitate in nitric acid, and titration with standard molybdate.

[The yellow phospho-molybdate has frequently been used as a basis of calculation in determining *small* quantities of phosphorus in analyses of iron and its ores. But never, to my knowledge, has it heretofore been successfully employed where large percentages of P_2O_5 were to be determined. Previous to the investigations of Dr. Gibbs, on the phospho-molybdates, many chemists were in doubt as to whether the yellow precipitate, with its extraordinary formula, actually was a definite chemical compound or not. Different analyses of it varied as much as 30 per cent. on the total P_2O_5 contained therein. Eggertz and Finkner showed how the precipitate could be formed so as to vary in per cent. P_2O_5 within moderate limits, and the phosphorus estimations in iron and steel were thereupon based upon the direct weighing of the yellow salt.

It appears to be an impossibility, however, to precipitate the pure yellow salt, with the ratio of MoO_3 to P_2O_5 *exactly* as 24 : 1, when the usual nitric acid solution of the molybdate is employed. Because the length of time during which the solution must stand in the heat, and the large excess of the precipitant required, cause more or less MoO_3 to fall with the precipitate.

It is very evident, therefore, if there is any tendency in the same direction when the *aqueous* molybdate solution is employed, that the volumetric process would be unreliable, and the error a radical one. Happily, however, there is no danger of this; there cannot possibly be an excess of the molybdate in the solution, except the fraction of a cubic centimetre at the very end of the titration. My original experiments, together with the subsequent experience of nearly three years, are corroborated by the above criticism of Prof. Caldwell, and they all go to prove the constancy and reliability of the process.

It may be worth noting, that this is the only known volumetric process

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*Since the publication of the above, Prof. Caldwell writes: "To what appears in this report I might add that quite a number of additional analyses of the same fertilizers have since been made by my students, and with as good results as they get by any other method. I always use the suction filter described in the paper that you will receive, and which I think Mr. Pemberton will like as an auxiliary to his method."

of determining phosphoric acid when iron or alumina are present, or when the solution is a nitric acid one.

The only drawback I have found—that of determining with quickness the final point of the reaction—has been very largely overcome by the happy idea of Prof. Caldwell, illustrated in his paper above.

It is evident that this little portable Bunsen filter can be used with advantage in many other volumetric methods of direct precipitation, and would doubtless give a practical nature to a variety of processes otherwise difficult or tedious.

HENRY PEMBERTON, JR.]

SIZE OF DROPS AND THE U. S. DISPENSATORY.

By PROFESSOR CHARLES F. HIMES, PH.D., Carlisle, Pa.

In the recent edition, the 15th, of the U. S. Dispensatory, among other modes of approximate measurement, in the administration of medicines given, is that by drops, and in calling attention to the conditions affecting the size of drops, the statement is made, that "the drops from a full bottle should be less than from one more or less emptied." There is no indication that the statement rests upon direct tests, and it is so decidedly at variance with the results of experiment, that it seems but proper that attention should be called to it. Other conditions remaining the same, drops diminish in size as the bottle is emptied, and to such a degree that any one can satisfy himself of the fact in a few minutes. The circumstance which first directed my attention to the subject will illustrate also the degree of variation in size. It was assigned to a student in the laboratory, as an exercise, to ascertain how far drops might be substituted for more precisely measured quantities of liquids in making comparative determinations for domestic or even commercial purposes. As a preliminary test of the degree of uniformity of results, the hardness of a constant quantity of the same sample of water was destroyed by dropping into it standard soap solution, from the same bottle, and the same portion of the lip. The number of drops of course varied, but, after some skill had been acquired in using the method, in a series of experiments, the number of drops reported as required, indicated regularly increasing hardness for four experiments, then a sudden diminution, succeeded by regular increase. Upon watching the procedure of the student, there seemed to be no condition varying with the same regularity except the amount of liquid in the bottle, and consequent size of the drops; and upon

performing the experiments by filling the bottle up to the same mark for each trial, the previous periodicity disappeared, and the uniformity of results was greater than had been anticipated. Subsequent experiments with different bottles, and different liquids, demonstrated that the differences in size of drops occasioned by the variation in the amount of liquid in a bottle were not such as might in all cases be overlooked with prudence in the administration of medicines, and that the method of drops, untrustworthy at best, was rendered much more so from this fact. Upon reference to an older edition of the Dispensatory, on hand at that time, no allusion to the effect upon the size of the drops of the amount of liquid in the bottle was made, and it seemed hardly necessary to call attention to a fact presumably known to any who had employed the method.

Since sending the preceding hastily prepared note upon the size of drops I have taken the opportunity to run over the literature of the subject as given in the *American Journal of Pharmacy*, not then at my command, and am surprised that, with the painstaking character of the investigation of the conditions affecting the size of drops, this one of the degree of fullness of the bottle is practically uninvestigated, and left among the undetermined conditions.

In 1830, Durand* called attention to a series of experiments by Dr. Shuttleworth, and their appreciation by the College of Physicians of London, and gave as the conclusion from experiments of his own, "that the practice of prescribing fluid medicines by drops is altogether objectionable," and that ignorance of the conditions affecting size of drops may lead to serious consequences. After summing up the usual conditions of density of liquid, cohesion of particles, shape of the mouth of the vessel, etc., he remarks, "besides, in every instance, the first drops poured from any vase are always smaller than those subsequently obtained." There is no indication whatever that he, in the most indirect manner, intends to allude to the condition of the bottle as to fullness, but simply to a fact, which the author has not verified, that the *few* drops *first passing* over the lip of the vessel are smaller than the subsequent ones.

In 1858, Bernoulli† gave results of investigations with great accu-

* *American Journal of Pharmacy*, vol. 1, p. 165.

† *Schweizer. Zeitschr. f. Pharm.*, 1858, pp. 97—100; *Am. Jour. Phar.*, vol. xxxi, p. 441.

racy, "keeping in view the nature of the vessel, the temperature, the rapidity of dropping, and other circumstances calculated to affect the weight."

In 1860, Proctor* calls attention to the well-established variation in size of drops, with form and size of vessel, with density and viscosity of liquids, "and according to some other of its qualities not yet well understood."

In 1864, Quin, in the *Druggist and Chemist*,† prefaces an abstract of very careful and exhaustive investigations by Guthrie of the conditions affecting the size of drops by a remark upon "the difficulty of obtaining a standard drop, a difficulty which is still more increased by the knowledge that even when the same vessel and liquid are used the differences are almost as great as those already cited."

Parrish, on experimenting with ounce vials of water, found, in seven trials, the drops required for a fluidrachm to vary between thirty-two and sixty-five.

Guthrie investigates most fully "the physical relations existing between the matter on which the drop is formed, the liquid constituting the drop itself, and the medium through which it passes," a condition not clearly expressed before; but, after enumeration of the other usual conditions, as given by Guthrie, Quin remarks: "The condition, however, which has the greatest effect upon the size of the drop is the interval which takes place between the successive drops, and called by Prof. Guthrie the *growth-time*."

The careful experiments by Prof. Guthrie upon the influence of this condition are quoted, with his conclusion that, "on the whole, the law seems to be, the slower the dropping the smaller the drop." This was regarded as a most interesting fact to the pharmacist, as showing "the influence of rate in dispensing drops."

The paper of Prof. Guthrie is highly commended for its exhibition of talents and patience in investigation, and this "enormous and evidently most fruitful field" of investigation commended to him. But although growth-time may in some way be involved in the degree of fullness of the bottle, in not the remotest manner is the latter condition alluded to.

In an editorial of the *American Journal of Pharmacy*, Feb., 1877, vol. 49, differences in size of drops, of from 30 to 100 per cent., from

* *Lon. Pharm. Jour.*, July 2, 1860; *Am. Pharm. Jour.*, vol. xxxii, p. 428.
† *Am. Jour. Phar.*, vol. xxxvi, p. 522.

the same vessel, is remarked upon, and the experiments of Durand, before mentioned, are alluded to.

In 1880, Talbot* gives an account of experiments upon this subject, alludes to those of Durand and Bernouilli, and, in summing up his results, gives the opinion that "the administration of powerful medicines by drops is always dangerous," that "a single bottle is inconstant" as to size of drops, that "cohesion exerts the greatest influence upon the bulk of drops, temperature very little effect, and rapidity of dropping almost none,"—the last conclusion being at variance with that of Guthrie, previously given.

In none of these papers has the condition mentioned and illustrated in my first note, been alluded to.

NOTES ON THE METALLURGY OF NICKEL IN THE UNITED STATES.

By WILLIAM P. BLAKE, F. G. S., New Haven, Conn.

[Read at the Boston Meeting of the American Institute of Mining Engineers, Feb., 1883.]

The metallic element Nickel, discovered by Cronstedt the mineralogist, in the year 1751, as a peculiar metal in kupfer-nickel, remained for a long time comparatively unknown in its true characters. It was at first obtained as a secondary or by-product, in the manufacture or extraction of cobalt, being found concentrated in the cobalt speiss left in the pots when smalt or cobalt-blue glass was manufactured. Cobalt at that time was the product chiefly sought, and nickel in its applications was unknown. Since the discovery of the artificial ultra-marine blue, the demand for cobalt has been lessened, while the increasing uses of nickel have made it of first importance, and the conditions are thus reversed.†

But the nickel so produced from the residues was contaminated with copper, iron or arsenic, and in this condition it entered into the composition of the familiar alloy commonly known as German silver, but properly known as nickel silver.

The so-called nickel or nickel bronze was a complex, irregularly constituted alloy, in which less than one per cent. of arsenic was sufficient to greatly modify its physical properties. And it was difficult to

* *Am. Jour. Phar.*, vol. lii, p. 337.

† See Daubrée, *Substances Minerales*, p. 158.

free the metal from this element. It may be said that until within a few years, the element nickel, in its true characters and in a comparatively pure condition, was commercially unknown.

To the scientific chemists, however, its true physical properties early became known, though not without some contradictory and varying results, at first, resulting no doubt from minute differences of composition of their samples according to the nature of the processes employed for the extraction of the metal. Richter found that nickel oxide strongly ignited in an earthen crucible with carbon, gave the metal in a perfectly malleable, ductile condition. It could be hammered cold or hot into plates $\frac{1}{100}$th of an inch in thickness, and could be drawn into wire $\frac{1}{16}$th of an inch in diameter.* Its malleability was found to be diminished by carbon or manganese. On the other hand, Tupputi found that nickel reduced in the presence of carbon in a covered charcoal crucible and under glass, formed more or less nickel-graphite, absorbed a portion of carbon, and was less ductile than zinc. It was brittle when cold, and was as fusible as cast iron (Erdman), while the metal obtained by Richter was difficult of fusion. He also noted that nickel could be welded, but Tourte found that it welded but imperfectly.

Deville cited cobalt and nickel as metals with useful physical properties but little known, such as malleability, ductility, and a tenacity supassing that of iron. He showed that these metals could be worked at a forge with the same facility as iron; that they were susceptible of being employed in the same manner and were less oxidable.†

Inasmuch as nickel first became known commercially in the industrial arts in the form of an alloy, there were no special attempts to produce the metal in a state of extreme purity. The nickel silver of commerce answered all the existing demands, and was of course much easier to make, and cheaper than the pure nickel. It found a large and rapidly extending consumption as a substitute for silver spoons and forks and for silverware generally, especially when the new art of electro-plating was developed by Spencer, Smee, and others. The nickel silver was specially well adapted to receive and hold the deposit of silver, and it is to this day the most desirable alloy for plating.

The use of nickel alloy for small or subsidiary coins next made an increased demand for nickel. Tentative efforts were made by Dr.

* Cited in Gmelin v. 361.

† Comptes Rendus de l'Academie.

Feuchwanger, in New York, in the year 1837, and he actually issued many small one-cent and three-cent pieces, made of a nickel alloy, the exact composition of which he was careful not to state, but called it "Feuchtwanger's Composition." Switzerland commenced using nickel alloy coins in 1850; the United States in in 1857, though sample coins, one-cent pieces, had been made by Prof. James C. Booth, at Philadelphia in 1853, the prepared alloy containing from 5 of nickel and 95 of copper, to as high as 30 of nickel, and 70 of copper. The alloy adopted by law consisted of 12 of nickel and 88 of copper. The five-cent pieces now in circulation are made of an alloy of 25 parts of nickel and 75 parts of copper. In 1860, Belgium adopted an alloy of the same proportions, for small coins. Other countries have followed, until the use of nickel alloy for small coins may be said to be almost universal in the chief commercial countries. Up to June 30th, 1876, the United States had alone issued of the five-cent nickel to the extent of $6,716,129 in value. Another sudden demand for a larger supply of nickel sprang up when the art of depositing nickel by electricity was perfected. The many and increasing applications of this art need not be here enumerated. It is sufficient to state that at the present time, they constitute a large part of the present consumption of the metal in this country, where the art may be said to have originated in a successful, practical form.

Nickel ore is more generally distributed throughout the mineral-bearing portions of the United States than is generally supposed. It is commonly associated with chrome ores from Canada to Maryland, on the Atlantic side, and equally with the chrome ores of the Pacific slope, notably in Oregon. It is also a common associate of magnetic pyrites in the Archæan rocks, being found in Litchfield county, in Connecticut; in the Highlands of the Hudson, in New York and in New Jersey; and especially at Lancaster Gap, in Pennsylvania, where the chief supply of nickel has been obtained for the United States. This ore yields from 1½ to 2 per cent. of nickel, but is enriched by smelting at the mine into a matte containing 10 per cent. or more of the metal. This locality was worked some thirty years ago by Prof. James C. Booth and others, of Philadelphia, and some nickel alloy was made. Some ten years later Mr. Joseph Wharton purchased the works and established the industry at Camden, N. J., opposite Philadelphia, where it has since been carried forward.

A large portion of the metal produced at these works by Mr. Whar-

ton has been used at the United States mint for the subsidiary small coins, and a considerable amount has been exported. Since the development of nickeling by galvanism, a large part of the product has been put into the form of nickel salts and anodes.

But Mr. Wharton not being content with the production of impure nickel, early commenced experimenting to determine whether nickel could not be produced in a pure and malleable condition, susceptible of being worked in nearly the same manner as iron, and of being applied in the manufacture of various objects requiring strength of material and a material that cannot be easily oxidized. One of his earliest experiments was to take the somewhat spongy mass got by reduction of the oxide of nickel, and after heating it to full redness, work it under a steam-hammer into a bar.

In 1873, Mr. Wharton sent to the Vienna Exhibition a sample of nickel in the form of axles and axle bearings, and at the exhibition in Philadelphia in 1876, he exhibited a remarkable series of objects made of *wrought nickel*, such as bars, rods, a cube, a horse-shoe magnet and magnetic needles of forged nickel. These did not excite the interest to which they were entitled as a remarkable advance in the working of this little-known metal. The exhibit did not cause much comment, and it was not specially described or reported upon, so far as I am aware, except by the judges who reported the exhibit to the Commission as worthy of an award in the following terms: "A fine collection of nickel ores from Lancaster County, Pa., with nickel-matte, metallic nickel in grains and cubes, and manufactured nickel, both cast and wrought; nickel magnets and magnetic needles, cast cobalt, electroplating with nickel and cobalt, and salts and oxides of both these metals; the whole showing a remarkable degree of progress in their metallurgical treatment."*

Some of the same objects formed of wrought nickel, were sent over to Paris two years later, and were exhibited in the American Section in 1878. There, as in Philadelphia, they did not at first excite any surprise, or receive any special attention. Very few persons realized what the objects really were, and that they were very different from *alloys* of nickel. In fact, very few chemists had ever seen *nickel*. Pure nickel was a rarity, a curiosity, just as samples of indium or thallium are to-day.

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*Reports and Awards, Group one, 640, p. 470.

You can then, perhaps, imagine the incredulity of the expert chemists and metallurgists of Europe, when whole ingots and forged bars of metal and numerous finished articles of pure wrought nickel, without alloy, were offered for their inspection. These articles not differing greatly in their appearance from the higher grades of nickel alloys, or from electro-nickeled objects, they passed them without surprise. No previous exhibition had been so rich in exhibits of the use of nickel and in the products from them. The influx of the pure carbonated and oxidized ores from New Caledonia, had greatly stimulated the nickel industry in Europe, and had improved the quality of the alloys of nickel. New companies had been formed to manufacture nickel silver and to produce nickel from these superior ores, at a lower cost than had before been possible. Christofle of Paris had just erected extensive works at St. Denis, and had made a most brilliant display of his products in one of the main avenues of the Exposition. The Vivians of Swansea and other exhibitors had large cases filled with beautiful objects of hollow and solid ware made of nickel silver. Amid these various exhibits of striking *tours de force*, the modest little showcase from the United States with examples of manufactures of *pure wrought nickel*, not alloy, could hardly be expected to excite attention and win the golden award, which was most cheerfully accorded as soon as the fact was demonstrated by analysis that the objects were really of the pure metal. Some of the objects now shown were at that exhibition, and have retained their brilliant polish and lustre unimpaired. These notable advances in the metallurgy of nickel, made with the lean and sulphuretted ores of Lancaster Gap, prepared the way for greater advances.

Dr. Fleitman of Iserlohn, Westphalia, Prussia, has improved and cheapened the operation of refining the nickel and toughening it, and has reduced the liability to the presence of blow-holes in castings by adding to the molten charge, in the pot, when ready to pour, a very small quantity of magnesium. This is immediately decomposed, magnesia is formed, and graphite is separated. It would seem that the magnesium decomposes the occluded carbonic oxide, or reduces it to a minimum. The magnesium must be added with great care, and in small portions, as it unites explosively with the charge. It is stirred in. About one ounce of magnesium is sufficient for 60 pounds of nickel. Three-quarters of an ounce to 54 pounds of metal has been used with success by Mr. Wharton. The nickel from the ore at Lan-

caster Gap, seems not to require as much as the foreign metal. It is to be noted that complete malleability of nickel was obtained at Wharton's Works in Camden, before Fleitman's invention or process, but this last is more rapid and better than the old method. The metal so treated becomes remarkably tough and malleable, and may be rolled into sheets and drawn into wire. Cast plates can be successfully rolled. The cast plates, such as are made for anodes, after reheating, are rolled down to the desired thickness. It is found that it is a great improvement to the nickel anode plates to roll them down. They dissolve with greater uniformity in the bath. Nickel so treated with magnesium has been rolled into sheets as thin as paper. Expensive works for rolling the metal have been erected by Mr. Wharton at Camden. There is already a train of 40-inch rolls, 18 inches in diameter, with annealing ovens and gas furnaces and their adjuncts, and a 90 horse-power engine. At present this mill as well as the works for producing the metal, and the mine also, are "shut down."

The largest sheet yet rolled at Camden was 72 inches long and 24 inches wide, of pure nickel.

Dr. Fleitman has also succeeded in welding sheet nickel upon iron and upon steel plates, so as to coat them equally on each face with a layer of nickel. The quantity preferred by weight is $\frac{8}{10}$ iron and $\frac{2}{10}$ nickel, one-tenth of nickel being placed on each surface. To secure union the iron or steel must be perfectly flat and clean. A pile is made with outer facings of sheet iron to protect the nickel from scaling. When the whole is heated to the proper degree, it is passed through the rolls. The two metals become so firmly united that they may afterwards be rolled down, two or three together, or separately, to the thinness desired.

The samples exhibited were cut from sheets made at Mr. Wharton's works at Camden. One sample No. 20 gauge, 10 per cent. nickel; one sample No. 22 gauge, 10 per cent. nickel; one sample showing edge of sheet.

These are all examples of nickel upon iron. I also show a thin sheet of pure nickel annealed. The physical properties of the two metals, iron and nickel, are so nearly the same that they work well together. The nickel surface cannot be removed or regained in the scrap and waste except by dissolving out the iron core by dilute sulphuric acid. In the earlier experiments, the ingots or cast plates were beaten under the hammer; this produced a great deal of scale and

waste, as with iron, but this is now avoided, partly by the device of a thin covering of sheet iron which is afterwards dissolved off. Dr. Fleitman claims to have produced steel wire similarly coated, and proposes to make nickeled boiler plates.

The applications in the arts of such nickeled iron sheets as I have described, will readily suggest themselves. Up to this time the most direct uses seem to be in making hollow ware, particularly culinary vessels. The manufacture has already begun at Schwerte, by Dr. Fleitman, and a great variety of vessels, such as saucepans and kettles have been turned out, some of them of pure sheet nickel. They are all very beautiful in appearance, resembling highly finished platinum vessels more than ordinary ware. When planished and buffed off, the surface becomes like a mirror, and will answer the purpose of one. The small vessel exhibited is made of nickeled iron, and will show the facility with which the compound sheet metal may be stamped, spun up, and polished. Much larger specimens of ware might be shown.

This ware is believed to be far superior to tinned iron or tinned copper for cooking in. The nickel is not only less liable to corrosion, but is harder, will wear longer, and cannot be melted off by overheating. The ware is lighter and stronger than tin or copper ware; is susceptible of a high polish and is not easily tarnished. It appears to be well adapted to the manufacture of dishes, salvers and covers for the table. The coating of nickel applied by welding is stronger and tougher than that deposited by electrolysis, and appears to be less liable to scale off. The electrically deposited metal is in some cases very brittle, and no doubt contains sufficient hydrogen to essentially modify the physical characters of the coating.

My purpose in this article is chiefly to record some of the most notable advances in the metallurgy of nickel made in the United States, and particularly to direct attention to the production by Mr. Joseph Wharton at his works in Camden, before the year 1876, of *pure nickel* in a malleable state and in considerable quantities, and the manufacture of useful articles from it by forging and working it in the same manner as iron is forged and worked; thus exhibiting for the first time, in the "large way," the true physical characters of this metal, and its adaptation to many purposes in the arts, as had already been partly indicated, in the "small way," by scientific chemists in their laboratories.

I also desire to direct attention to the improved magnesium process

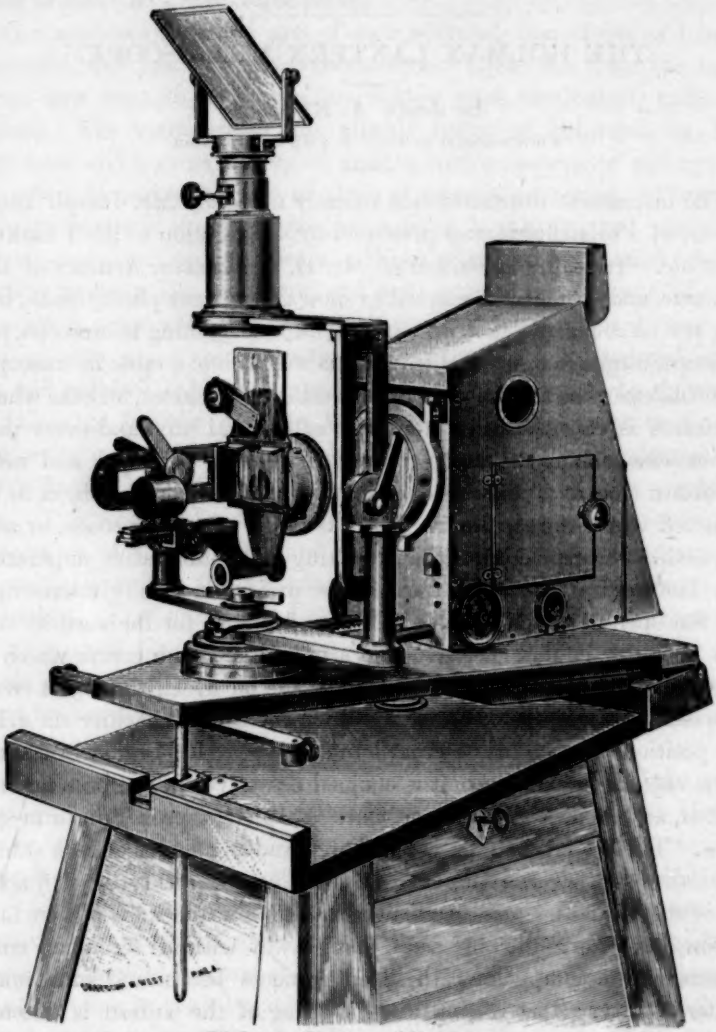
of Dr. Fleitman; to the manufacture of nickeled iron and steel in rolled sheets, and to the industries which the possession of pure nickel in commercial quantities renders possible.

THE HOLMAN LANTERN MICROSCOPE.

By JOHN A. RYDER,
Embryologist to the U. S. Fish Commission.

The instrument illustrated was recently made by Mr. Joseph Zentmayer, of Philadelphia, and presented by subscription to the Franklin Institute. It is the invention of Mr. D. S. Holman, Actuary of the Institute, and is not only adapted to show transparent photographs, but may also be converted, in a few minutes, into a projecting microscope, polariscope, megascope, vertical lantern, as well as into a table microscope. The condensors of the lantern are five inches in diameter, and the whole apparatus is constructed in the most substantial way, and every part and accessory is of the best workmanship. The principal and most important feature of the apparatus consists in making the object to be projected when the apparatus is to be used as a microscope, or as a megascope, the centre of all the projecting and illuminating apparatus. The lantern upright which carries the projecting lens or microscope, and the upright which carries the lens and mirror for the vertical lantern and the lantern itself, swing around a common centre which is placed exactly below the centre of the stage on which the object rests. This enables the operator to arrange with the greatest facility the relative positions of each of these parts in any desirable position in respect to the object. This feature it is claimed has never before been accomplished, and makes the lantern more complete, especially as a megascope. The lantern and jet are movable, independently of each other, by means of racks and pinions. By this arrangement the conjugate foci of the condensor may be changed to suit low and high powers and narrow and wide angle objectives, doing away with all secondary condensors and accomplishing these adjustments better and with much greater facility. The raising and lowering of the lantern is effected by a peculiarly constructed clutch, which rigidly holds the instrument in any inclined position. The instrument is packed in a truncated pyramidal box, containing also the accessories, and forming at the same time a firm stand for the lantern, on which it may be rotated

when in use. In consideration of the fact that the instrument can be used for such a variety of purposes for lecture illustrations, it cannot be regarded as other than a marked improvement upon a similar class



Holman Lantern Microscope.

of instruments hitherto used for such purposes. In the illustration and projecting of living forms alone, in conjunction with a number of ingenious devices serving as live cages, Mr. Holman has done a real

service for the cause of education. While it is perhaps not possible to enter into an elaborate or detailed study of any organic forms of even a moderate degree of complexity, if projected only for a few moments upon a screen, it is nevertheless a fact that the correct likenesses of such creatures, so shown, gives the beholder a far truer appreciation of what the things are of which he reads in books than he might possibly obtain elsewhere, provided the lecturer is able to explain in a lucid manner and unravel the complicated life histories of the living beings of which he displays enlarged images. In these living animal or perhaps plant pictures we have displayed two classes of facts, namely, those of type and those of function. To the trained biologist they call to mind the occult processes of growth and reproduction by which the forms become what we see them to be. This implies that a wide range of data is to be considered; first, there is the development and evolution of the form, together with what this indicates as to its systematic relationship; second, the vital actions displayed involve the consideration of physiological processes, and these again those internal quasi-chemical and physical actions and interactions by means of which the creature is enabled to maintain its existence and individuality. If such problems are not worth elucidating, we may ask what others there are which are worthy of elucidation. The physiologist who solves the problem of the life actions of an amœba or a maggot has put us in a fair way of appreciating those of a man. If, therefore, better and clearer ideas of animal existence can be fostered in young minds by the aid of any optical appliance whatsoever, that appliance should be welcome as an aid in practical objective instruction. No less effective is this instrument in the illustration of many common facts in physical science. The range of its applicability seems indeed to be limited only by the resources, ingenuity and ability of the lecturer.

Electric Navigation.—Sylvanus P. Thomson describes the new electric boat which has lately been tried upon the Thames. The experiments have shown that the electric machinery is both lighter and more compact than steam machinery of a corresponding power, that the noise and the vibrations are largely reduced, and that it completely suppresses all the inconvenience of smoke, which appears inseparable from steam navigation.—*La Nature*, Oct. 21, 1882. C.

Nickel Crucibles.—M. Mèrmet recommends the use of nickel crucibles instead of silver, in chemical manipulations. They are slightly attacked, it is true, by melted potash, but silver itself is not indifferent to this action. They cost at first much less than silver, and moreover they have the great advantage of melting at a higher temperature. It often happens, in fact, that inexperienced chemists melt their silver crucibles in heating them over a gas lamp; such an accident is not to be feared with nickel crucibles.—*Chron. Industr.*, Feb. 25, 1883. C.

Theory of Electromagnetic Machines. — Tresca's experiments with Deprez's machines give, for the first time, the value of the well-known but unexplained loss of work. This loss has been attributed to different causes. Joubert thinks that the following is the most important. All machines with continuous currents are composed of a certain number of elements, such as the strands of the Gramme ring, which, when the machine acts as a receiver, pass from a position in which the potential energy has a maximum value W_0, to another, diametrically opposite, of minimum value W_1. The difference $W_0 - W_1$, represents the electromagnetic work furnished by the strand in passing from the first position to the second. In order that the movement may continue, it is necessary to reverse the direction of the current in the strand, or, in other words, to destroy the electric energy which it possesses, and restore the primitive energy W_0. This operation is repeated twice in each revolution for each strand. It is known, experimentally, that in the receiving machine the change of current must be made before the strand passes the line of the poles. The position of the commutator is given analytically by the condition that the electromagnetic work, $W_0 - W_1$, must be a maximum. Like considerations may be applied to the machine when working as a generator. All the coefficients which enter into the equations can be determined directly. The verification of the theory is, therefore, comparatively easy. It is indirectly confirmed by the values which Tresca has given. The value which they give for the coefficient of self-induction in Deprez's machines is of the same order of magnitude as the observed value. [Chase's discovery that the work of gravitation, at sun's surface, during a half rotation, gives the velocity of light, and Webber's electromagnetic ratio, lend additional interest to this theory.]—*Comptes Rendus*, March 5, 1883. C.

Rainbow Glass.—The soap-bubble colors upon glass are produced by a vapor, which is deposited on the hot glass before it goes into the annealing oven. The vapor comes from a mixture of protochloride of tin, carbonate of baryta, and carbonate of strontian. It is said that the workmen of a Bohemian manufacturer, wishing to celebrate his arrival, kindled some Bengal lights in the annealing furnaces, and the pieces which were in the furnaces all became iridescent. The colors can be removed by hard rubbing. Messrs. Clémandot and Fremy produced a pearly lustre, like that of shells, by means of different chemical agents, chlorhydric acid among others, under pressures of four, five or six atmospheres. They were thus able to imitate the magnificent glasses of antiquity, which have become iridescent through the lapse of time.—*Bull. de la Soc. d' Encour.* C.

Photographing the Solar Corona.—Huggins has succeeded, by the aid of colored glasses and solutions of potassic manganate, in photographing the corona. Using a Newton 3-inch telescope, with a focal distance of 3½ feet, he obtains a very distinct photographic image of the sun, without any perceptible defect. When the exposure is very rapid there can be seen around the sun, under suitable conditions of illumination, the interior corona, which is more regular in form than the exterior corona, and which extends about a quarter of a solar diameter beyond the sun's image. An exposure a little less rapid produces a reversal of the photographic image of the sun; the interior corona is absorbed in the exterior, which appears black, as in a negative proof. If the exposure is a little longer, the reversal is produced both in the solar image and in the corona, but not in those parts of the plate which are affected by the atmospheric light alone. Afterwards the plate represents the corona in white, as in a positive proof, and the atmospheric light in black. Then the straight and curved lines, and the other varied forms which characterize the corona, can be easily traced. Capt. Abney has carefully compared these plates with those which were taken during the last total eclipse. He does not doubt that Huggins is able to photograph the corona at any time, without waiting for eclipses which occur only fifteen times in a century. The new method will enable observers, especially in southern climates and at high elevations, to make this phenomenon, which is one of so great importance in solar research, the subject of daily study.—*Comptes Rendus*, Jan. 2, 1883. C.

A New Food.—According to the *Chemiker Zeitung*, M. Muller has evaporated skimmed milk in a vacuum, so as to obtain a permanent product, which can be preserved for many months in a dry atmosphere, and which has valuable alimentary properties. He thinks that it may be of great use in pastry, and in various kinds of baking, and the best sugar of milk can be made from it. The skimmed milk which is collected in dairies and cheese factories is usually given to animals or wasted in sewage; it contains, however, large quantities of salts, and particles of butter and casseine, which can be utilized by Muller's method.—*Rev. Scientif.*, Jan. 6, 1883. C.

Influence of Fluids upon Sound Waves.—Felix Auerbach publishes the following experimental results: 1. The geometrical lowering of tone, which a fluid produces in a cylindrical glass which it completely fills, diminishes in magnitude with the height of the tone of the empty glass. 2. The arithmetical lowering which a fluid produces in a cylindrical glass of medium register is nearly inversely proportional to the square root of the number of vibrations of the empty glass. 3. The lowering which a fluid produces in a cylindrical glass which it completely fills is not perceptibly dependent upon the height of the glass. 4. The geometrical lowering in cylindrical glasses of different diameters is greater the narrower the glass. 5. The arithmetrical lowering is approximately inversely proportional to the square root of the diameter of the glass.—*Ann. der Phys. u. Chem.*, Nov. 15, 1882. C.

Use of Liquefied Gases.—In Krupp's establishment compressed carbonic acid is used for the manufacture of ice and of seltzer water. Dr. Raydt has taken out patents for drawing beer under a pressure which is produced by liquid carbonic acid. Major Witte has provided the steam fire engines of Berlin with pipes for the discharge of compressed carbonic acid into the steam chamber. When the engine starts from the station the boiler is heated; on arriving at the fire the carbonic acid is first employed as a motor, then the gas and the steam work together, and finally steam alone is used. By this arrangement the engine is brought into action four or five minutes sooner than would be otherwise possible. The consumption of the liquid gas is about 8 kilogrammes; twice the amount should be taken, and in two receptacles, to allow for the portion which congeals in cooling—*Rev. Scientif.*, Jan. 6, 1883. C.

Tea Culture in France.—Experiments have been undertaken to acclimatize the tea plant in the department of Loire-Inférieure. Some grafts upon camelia stocks have been subjected in the open air to a temperature of 18° below zero (— ·4°F.). In Sicily, near Messina, similar experiments have been continued for three years; 120 shrubs have been exposed in the open field, through the entire winter, and are all flourishing. The leaves are of as good quality as the Chinese. The shrubs are planted like vines, and yield two crops per annum.—*Les Mondes*, Dec. 2, 1882. C.

Congelation of Solvents.—If A designates the lowering of the point of congelation due to the presence of one part of any soluble substance in 100 parts of the solvent; M, the molecular weight of the dissolved substance; T, the lowering of the point of congelation by a dissolved molecule; F. M. Rault finds that MA = T. From his experiments he derived the following conclusions. Every body, when dissolved in a liquid which is capable of solidification, lowers the point of congelation. In all liquids, the molecular reductions of congelation which are due to different compounds approximate to two values, invariable for each liquid, of which one is double the other. The normal molecular reduction of congelation varies with the nature of the solvent. A molecule of any compound, on being dissolved in 100 molecules of any liquid, lowers the point of congelation by the nearly constant quantity 0°·62 (1·°12 F.)—*Comptes Rendus*, Nov. 27, 1882. C.

Vision of Ultra-violet Rays.—Mascart thinks that the views of de Chardonnet, with respect to the office of the crystalline lens in intercepting the ultra-violet rays, require some qualification. Undoubtedly the humors of the eye absorb many of those rays, but without intercepting them completely, and the retina is so delicate an organ that it may be sensitive to the least radiations which escape entire absorption. By employing a spectroscope of quartz or of Iceland spar he found, in 1869, that ordinary eyes see the ultra-violet spectrum of a grayish lavender color. M. Isambert, who appeared to have a peculiarly sensitive retina, was able to chart the rays of cadmium vapor in the Iceland spar spectrum, at an angular distance from H, seven times as great as the luminous spectrum. The wave length in this region is about 210 millionths of a millimetre, a limit which Mascart has not been able to reach even by direct photography.—*Comptes Rendus*, Feb. 26, 1883. C.

List of Books Added to the Library during April, May, and June, 1883.

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Agriculture and Public Works for Quebec. General Report of the Commissioner for 1880. Presented by L. S. Ware.

American Agricultural Association. No. 1, Vol. 1. Presented by L. S. Ware.

American Chemical Society; Journal. Vol. 3. 1881. Presented by L. S. Ware.

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Annual Cyclopædia for the year 1882, N. S. Vol. 7. New York, D. Appleton & Co., 1883.

Archives du Musée Teyler. Second Series, 2d and 3d Parts. Presented by the Managers.

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Atwood, G. Propositions determining the Positions of Floating Bodies and the Stability of Ships, 1796. Presented by the late John Lenthall.

Berry A. Etude sur la Determination Rigoureuse de la Resistance des Larènes. Paris, N. D. Presented by the late John Lenthall.

Biological Lectures delivered to the Employés of the Baltimore and Ohio Railroad Company. Presented by J. W. Garrett.

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Bureau of Education. Answers to Inquiries about the Bureau; Its Work and History. By Charles Warren. Washington, 1883. Presented by the Bureau.

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